THE OCTOBER SCIENTIFIC MONTHLY

EDITED BY J. MCKEEN CATTELL

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Tuberculosis. GERALD B. WEBB. xv + 205 pp. Illustrated. $2.00. Paul B. Hoeber (Harper).

This book, one of the "Clio Medica" series, contributes to the history of medicine the history and treatment of tuberculosis. There are classic illustrations and charts that show the spread of the disease during the past few centuries.

The Truth Concerning the Invention of Photography—Nicéphore Niépce. VICTOR FOUQUE. Translated by Edward Epstean. 163 pp. Tennant and Ward.

A history of the invention of photography and a biography of its inventor, Joseph-Nicéphore Niépce, of his life, his endeavors and his works from his correspondence and other unpublished documents. The author claims that the invention of photography has been unjustly attributed to Daguerre.

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REMINISCENCES

BY

GEORGE HENRY FOX, A.M., M.D.

Formerly Professor of Dermatology, College of Physicians and Surgeons, Columbia University, New York.

Excerpts:

Hebra was a most remarkable man and in his daily clinics showed a keenness of observation which was simply wonderful. He rarely asked his patient any questions, but a hasty glance would usually enable him to tell more about the case than any one of his students would elicit by a lengthy catechism. Taking the patient's hand in his and looking straight at the class he would say "this man is a tailor" and we soon found that he discovered this fact by feeling the needle pricks on the roughened forefinger. A hatter he would recognize by some peculiar callus on the ball of the thumb and whether a patient was a confirmed beer or brandy drinker he knew by the appearance of the nose and the velvety condition of the skin. He could always guess the age and weight of a patient with unerring accuracy and tell us from what province of Austria he came. He could usually tell about how long an eruption had existed and repeatedly told his pupils not to ask whether an eruption itched but simply to look carefully and note the presence or absence of scratch marks.

One day a patient came in limping, and sat down to remove some bandages from his legs. While patiently waiting, Hebra casually glanced at him and remarked, "This man is a Croat, 55 years of age, has pulmonary tuberculosis and is a tailor by occupation." As he had not had a chance to see or feel his forefinger some of the students smiled at the statement, observing which he quickly asked, "wouldn't you know he was a tailor? Look at that little strip of drab cloth he has tied around the bandage. Tailors use that stuff for vest linings."

His power of imitation and of vivid description fully equalled his keen observation. One day a deaf mute came to the clinic and Hebra took occasion to refer to what was being done in Vienna for the unfortunate class by means of lip study. He said that children learned in a few months, to speak plainly but that their voices always had a peculiar lack of accent and intonation. He then gave an imitation of their speech and as I had visited the institution the week before I was amazed to note that the imitation was absolutely perfect. Another day a stupid boy, asked as to his occupation, said that he was a tailor. "Du bist kein Schneider," roared Hebra, "you are not even an apprentice. You pick up the litter at night and sweep out the shop in the morning. You go out for the beer at midday and rock the cradle when the Frau is preparing the dinner. That is the kind of a tailor you are." In short, with a few rapidly spoken sentences he gave us a word picture of that boy's habits and doings which was doubtless as accurate as it was amusing.

Vidal was a handsome and affable man but I acquired a dislike for him through no fault of his own. One day he exhibited a patient to a small group who were walking the wards with him and suddenly called on me for a diagnosis. Surprised and flustered I glanced at the numerous and disseminated small papules and hastily expressed my first thought "eczema." It happened to be a typical miliary papular syphilide and the expression on his face seemed to me to declare in loud tones that I was a hopeless ignoramus. I felt that I had not only injured my own unmade reputation but had utterly ruined that of the Vienna school. I walked home sad and dejected. My intense chagrin over my blunder almost banished sleep that night and for days and weeks I never saw M. Vidal at a distance without feeling that he recognized me as that young and foolish chap from Vienna who didn't know the difference between eczema and syphilis. Since that time I have made many mistakes in diagnosis but all of them put together have never given me serious discomfort or disturbed my sleep.

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J. McKEEN CATTELL, Editor

WARE CATTELL, Associate Editor

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The Psycho-Biology of Language. GEORGE K. ZIPF. ix + 366 pp. $3.50. Houghton Mifflin Company.

This book by a member of the Harvard Faculty will appeal to anyone interested in language as a form of human behavior. Its findings are so conclusive and significant that they may inaugurate an entirely new approach to the study of normal and abnormal personality.

Alaska Natives. H. DEWEY ANDERSON & WALTER C. EELLS. xvi + 472 pp. Illustrated. $5.00. Stanford University.

A report of sociological and educational investigations made by the author for the United States Office of Education, with a view to improving the native schools. It gives a picture of the Alaskans past and present, and their outlook for the future.

The Relativity Theory Simplified and the Formative Period of Its Inventor. MAX TALMEY. xi + 186 pp. Illustrated. $1.85. Darwin Press.

An explanation of the theories of relativity for the intelligent layman, with a brief biography of Einstein. The introduction has been written by Dr. George B. Pegram, professor of physics at Columbia University.

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Psychology. GLENN DeVERE HIGGINSON. xiii + 646 pp. Illustrated. $2.90. Macmillan.

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Random scientific observations of the water spider and the commonplace pigeon. The author combines the factual and the philosophic points of interest in the treatment of his subject matter. The kinds of species, their physical appearance, and general characteristics are described.

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THE SCIENTIFIC MONTHLY

OCTOBER, 1936

THE PHYSIOLOGY OF LIFE AT HIGH ALTITUDES

THE INTERNATIONAL HIGH ALTITUDE EXPEDITION TO CHILE, 1935

By Dr. ANCEL KEYS

ORGANIZER AND MANAGER OF THE EXPEDITION; ASSISTANT PROFESSOR OF BIOCHEMISTRY, THE MAYO FOUNDATION FOR MEDICAL RESEARCH

As one rises above the level of the sea, the pressure of the atmosphere (barometric pressure) falls; at 18,000 feet it is only about half as great as at sea level. The *composition* of the air, however, does not change appreciably and air is about 21 per cent. oxygen at all levels. This means that, at 18,000 feet, a given breath of air will only supply half as much oxygen as at sea level, a difference which must be expected to have profound effects on men and animals which require oxygen for life. Fig. 2 shows the relation between altitude and air (and oxygen) pressure, and gives a few high altitude landmarks.

In the early decades of the sixteenth century the Spanish Conquistadors invaded and despoiled the kingdom of the Incas. From the high plateaux of Peru and Bolivia they sent back tales of a fearful poison in the air of these mountains and of a dread disease, the *"sickness of the Andes,"* or "soroche,"[1] which no physician could cure. Breathlessness had long been known in certain cantons in Switzerland, but people in Europe did not experience sensations comparable to those of the South American explorers until the first attempts to climb Mt. Blanc (eighteenth century).

Scientific interest in the problems of life at high altitudes might well have begun with Lavoisier's demonstrations of the rôle of oxygen in the maintenance of life (latter part of the eighteenth century). However, aside from murdering a few mice in evacuated bell jars, little was done until balloon ascents captured attention seventy years ago. The French physiologist, Paul Bert, attempted to bring high altitude into the laboratory by studying men in vacuum chambers. Other investigators argued that, since the effect of low air pressure seems to be primarily owing to reduced oxygen pressure, high altitude can be simulated at sea level pressure by replacing some of the oxygen in the air by the inert gas nitrogen. Both types of chambers are still being used, but the abnormality of life in such chambers makes them of limited utility except for acute experiments. The immediate results of sudden reduc-

[1] The Quechua word "soroche" has gained some currency owing to the prevalence of the condition in the Andes and because for several centuries it was thought, in Europe at least, that severe mountain sickness is peculiar to the highlands of South America. In parts of Chile and Bolivia the term "puna" is used.

CERRO DE AUCANQUILCHA FROM THE VILLAGE OF OLLAGÜE

THE ALTITUDES OF THE THREE PEAKS VISIBLE ARE, FROM LEFT TO RIGHT, 20,150, 20,280 AND 20,100 FEET. SEE PHOTOGRAPH ON PAGE 306 FOR APPEARANCE OF THE LEFT AND CENTER PEAKS AS SEEN FROM THE RIGHT PEAK.

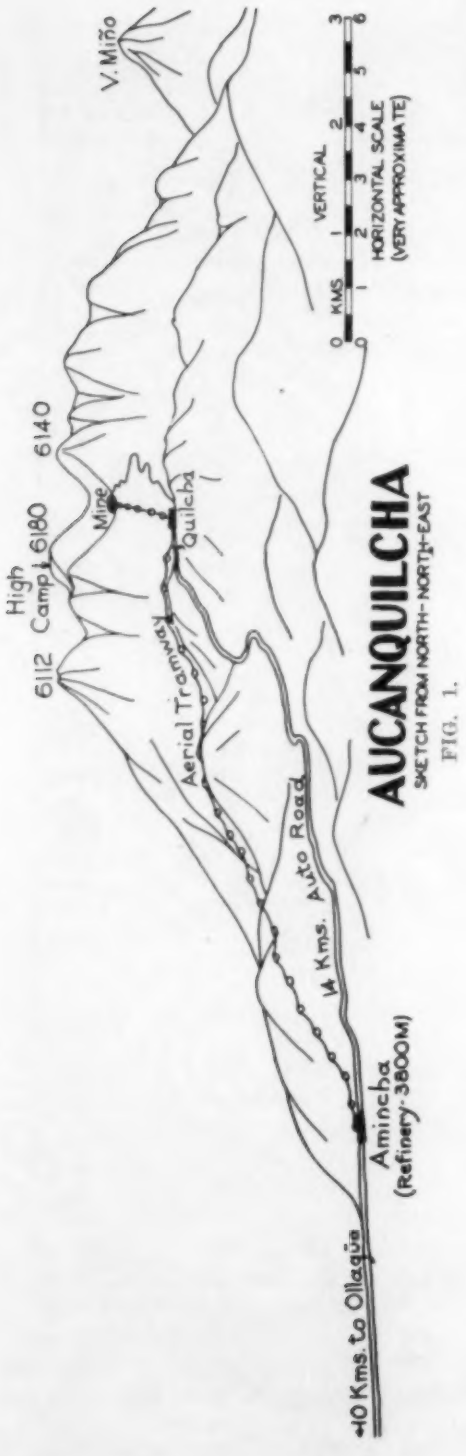


FIG. 1.

tion in air pressure are relatively easy to explain; the restoration of physical and mental vigor after some time at high altitude and the delayed onset of altitude sickness are both more interesting and less intelligible.

In these days there should be no need to dwell on reasons for making a study of the adaptation to life at high altitudes.

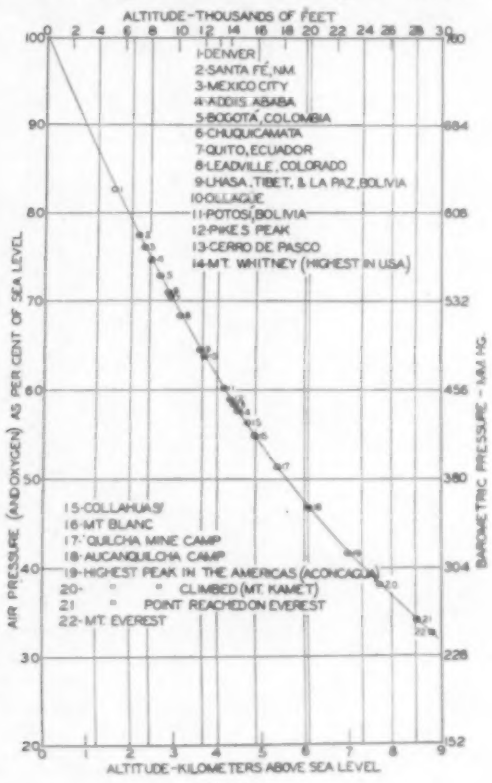


FIG. 2.

THE RELATION BETWEEN AIR (AND OXYGEN) PRESSURE AND ALTITUDE. THE NUMBERED POINTS ALONG THE CURVED LINE REFER TO THE VARIOUS PLACES TABULATED.

Each day brings new stories of high flying records, stratosphere balloons and plans for commercial transportation in the sub-stratosphere. Prospectors are finding more mineral deposits to be exploited in high mountains. As this article is being written, the latest Mount

FIG. 3.

MAP OF THE REGION IN NORTHERN CHILE IN WHICH THE INTERNATIONAL HIGH ALTITUDE EXPEDITION WORKED IN 1935.

Everest Expedition is preparing its base camp for another great effort which the world will follow with sympathetic interest. In all these enterprises the low air pressure of high altitude, with the corresponding lack of oxygen, is a major hazard.

In the civilized world it is now agreed that the physiological peculiarities of life at high altitude result almost entirely from the diminished oxygen pressure in the air. At high altitude one also finds intense cold and very great radiation from the sun, especially of ultra-violet light, but these are only small modifying factors; one can easily develop plenty of mountain sickness in a warm house at high altitude.

THE EXPEDITION TRAIN AT OLLAGÜE.

NOTE STEPS UP TO THE LABORATORY CARS.

The association of mountain sickness or soroche with a definite geographical place is easily understood. It is common in the Himalayas and in the Andes to be told that certain regions "contain much mountain sickness," or that, of two high peaks or passes of similar height, one "has much more soroche" than the other. In some cases there may be sound reasons for these statements, but the causes seem to be simply in differences in the terrain—that is, in the muscular exertion required to travel or in the speed with which altitude may be gained. For example, the Andes have an unenviable but merited reputation for producing more "soroche" than equally high regions elsewhere; the cause is probably that in the Andes one often ascends rapidly from the lowland to great heights.

Visitors to Pike's Peak know that the trip from 6,000 to 14,008 feet in a few hours is apt to leave one gasping and gazing about with "dull and jaundiced eye," and that a stay overnight is often attended with severe headache, sleeplessness and, not rarely, nausea. Many feel little at first but fall ill after a few hours, some never suffer anything worse than breathlessness, while the caretakers of the inn seem to be quite normal unless they attempt strenuous exercise. These differences illustrate two of the most interesting questions about high altitude effects, individual differences and acclimatization.

It is well known that many otherwise

BOLIVIAN TYPES

PREDOMINATE IN THE MOUNTAIN REGION OF NORTHERN CHILE NEAR THE BOLIVIAN FRONTIER. THE WHITE HAT AND SHAWL ARE CHARACTERISTIC.

healthy people are unable to stand high altitude and that as one approaches really great heights individual differences are accentuated. Moreover, aside from the recognition that people with definite cardiac or respiratory weakness are unfitted to go high, attempts to predict the responses of individuals to life at high altitudes heretofore have been attended with little or no success.[2] All these points were very clear on our expedition to Chile.

Mountaineering parties and explorers have demonstrated that by acclimatization man is able to live and to work at altitudes where continued existence otherwise would be impossible. The summer vacationer who spends a few days or weeks at eight or ten thousand feet considers himself rather hardy, but large communities at these altitudes are not rare. The mountaineer who, purple and gasping, drags himself to the top of a major peak in the Alps does not always remember that there are thousands of people in Asia and in South America who make their homes and carry out their daily work at an equal altitude. The aviator flying to above 20,000 feet without an artificial supply of oxygen suffers anything from mild malaise and lack of coordination to complete mental and physical collapse;[3] at the same and higher altitude the acclimatized mountaineer may feel almost entirely normal at rest and is able to do arduous physical work for hours with only moderate discomfort.

[2] Predictions as to ability to *fly* high for a *short time* are often relatively accurate.

[3] Physical exertion intensifies the danger but, even when sitting quietly, loss of consciousness followed by death can result, especially above 23,000 feet. Only good luck allowed Tissandier to return alive from the balloon flight in 1875 on which his two companions died at a little over 25,000 feet.

Knowledge of physiology and physiological theory would predict the general nature though not the precise form of the discomfort and danger of rarefied air, but gives very little clue to the acclimatization process. In this respect there are similarities with chronic heart disease[4] in which also physiology can tell why the patient is incapacitated but not why, in many cases, "compensation" is accomplished so that the sufferer is able to lead a nearly normal life. Perhaps the resemblance is more than superficial; in both cardiac disease and high altitude, deficiency of oxygen in the blood (anoxemia) is responsible for most of the unpleasant symptoms.

It will be agreed, then, that the problem of acclimatization to life at high altitude is of much theoretical and practical interest. For this reason low-pressure and low-oxygen chambers have been built in England, Denmark, Germany and the United States, laboratories have been established in Switzerland and Italy and special expeditions have gone to the Alps, to Pike's Peak, to Madeira, to the Caucasus and to Peru. Physiologists have turned mountaineers and have made innumerable experiments on themselves. These researches have not been unfruitful, but they have further emphasized the interest of the problems and the incompleteness of our knowledge.[5]

In the first place, it is clear that man at high altitude needs as much oxygen as at sea level and that when he exerts his muscles the necessary increase in oxygen consumption is about the same in both cases. Primary interest has centered in the means whereby a normal amount of

[4] Including the various forms of cardiac insufficiency but not conditions like angina pectoris.

[5] The history of the earlier work can not be summarized here; for an introduction refer to Joseph Barcroft, "Lessons from High Altitudes," 1926, Cambridge; A. Loewy, "Physiologie des Hohenklimas," 1932, Berlin; J. S. Haldane, "Respiration," 1935, New Haven.

THE SALAR DE OLLAGÜE

NEAR THE GREAT SALAR DE UYUNI. ALTITUDE 12,000 FEET.

oxygen is extracted from an oxygen-poor environment and is carried to the tissues of the body. A second set of questions is concerned with the ability to perform physical and mental work at high altitudes. Finally, we ask what differentiates the unacclimatized from the acclimatized man and what determines the extent of acclimatization possible to a given man.

Early in 1934 it was decided to make a new and more ambitious attack on these problems. The earlier work had shown that a stay of at least several months in the field should be provided for if true acclimatization was to be developed in the members of the expedition who were to be the principal experimental subjects. It was agreed that really great heights should be attained because the phenomena of adjustment are most remarkable above 15,000 feet. The most hopeful lines of attack seemed to be to concentrate on the chemistry of the blood and its ability to carry oxygen, on the performance of the heart and lungs, and on the responses of the body to various standard physical and mental tasks.

These considerations guided the organization of the International High Altitude Expedition. A relatively large party of robust investigators was needed to provide the varieties of technical knowledge, as well as the number of skilled analysts, required for the complex experiments planned. With the help of Professors L. J. Henderson, Sir Joseph Barcroft and August Krogh the personnel of the expedition was enlisted from the faculties of the universities of Harvard, Copenhagen, Cambridge, Duke, Columbia and Chicago; the ten men chosen embraced the fields of physiology, biochemistry, zoology, medicine and psychology.[6] Financial support was guaranteed by the universities and various research foundations and societies.[7]

Our program called for precise experiments and analytical procedures only possible with elaborate apparatus. Moreover, we wished to reduce to a minimum extraneous factors of discomfort which might tend to confuse our results. High altitude locations suitable to our needs are not numerous and are confined to two regions: Central Asia and the Andes of South America between about 10° and 30° S. latitude. We finally selected the Chilean Andes on the Bolivian frontier near the line of the Antofagasta-Bolivia Railroad. The region is shown on the map (Fig. 3).

A period of some months was occupied at the Fatigue Laboratory with assembling equipment and making a thorough study of the members of the expedition at sea level. The first half of the expedition sailed from New York on March 22, 1935, arriving at Antofagasta on April 7, and proceeding to Calama and Chuquicamata the next day. Two weeks later Matthews arrived from England and the last four members of the expedition arrived in Chile on May 18. Table I shows the itinerary in Chile.

By the newcomer, breathlessness may be scarcely noticed at Calama, but at Chuquicamata (9,200 feet) it is much more marked, though there is only a dif-

[6] In addition to the writer, the party consisted of: Drs. E. S. Guzman Barron, E. Hohwü Christensen, David Bruce Dill, H. T. Edwards, W. H. Forbes, F. G. Hall, Ross A. McFarland, Bryan H. C. Matthews, John H. Talbott.

[7] The expedition was supported by grants from the Fatigue Laboratory and the Milton Fund, Harvard; Duke University; Copenhagen University; King's College, Cambridge University; Columbia University; the National Research Council; the Royal Society, London; the Corn Industries Research Foundation; the Rask-Ørsted Fund and the Carlsberg Foundation, Copenhagen; the Josiah Macy Foundation and the American Association for the Advancement of Science. We are also much indebted to the Chile Exploration Company and its officers in New York and in Chile, to Señores Hipolito and Juan Carrasco of the S. I. A. M. Carrasco, to Messrs. Packard and Bell, of the Poderosa Mining Company, to the Ferro-Carril de Antofagasta a Bolivia, and to the many people, officials and private citizens who facilitated our work in Chile.

FOOTBALL AT HIGH ALTITUDES.

THE TEAMS FROM THE SULFUR MINES MEET AT OLLAGÜE (12,020 FEET). TEAM FROM THE 'QUILCHA CAMP (17,400 FEET).

FOOTBALL TEAM FROM THE SANTA ROSA MINE CAMP (16,400 FEET).

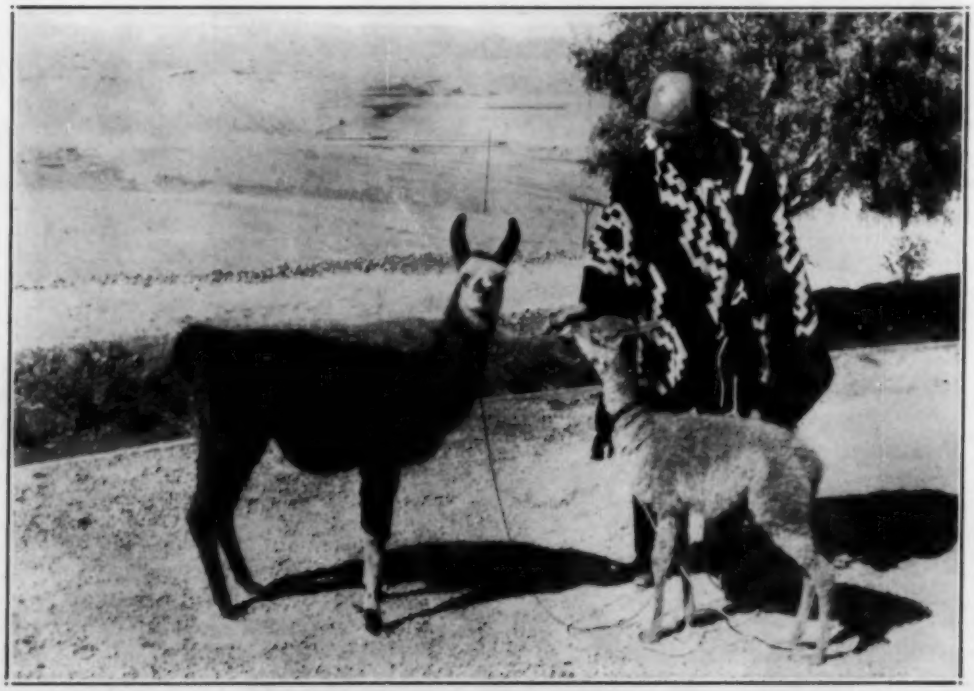
TABLE I

EXPERIMENTAL STATIONS ESTABLISHED BY THE INTERNATIONAL HIGH ALTITUDE EXPEDITION

Station	Period of occupation	Corrected barometric pressure	Altitude	
		mm mercury	meters	feet
Chuquicamata	April 8 to June 4	543	2,810	9,200
Ollagüe	June 5 to June 13 and June 25 to July 18	489	3,660	12,020
Collahuasi (Montt)	June 13 to June 25	429	4,700	15,440
Aucanquilcha	June 26 to July 15	401	5,340	17,500
Punta de Cerro	June 29 to July 14	356	6,140	20,140
Last departures from Chile August 17				

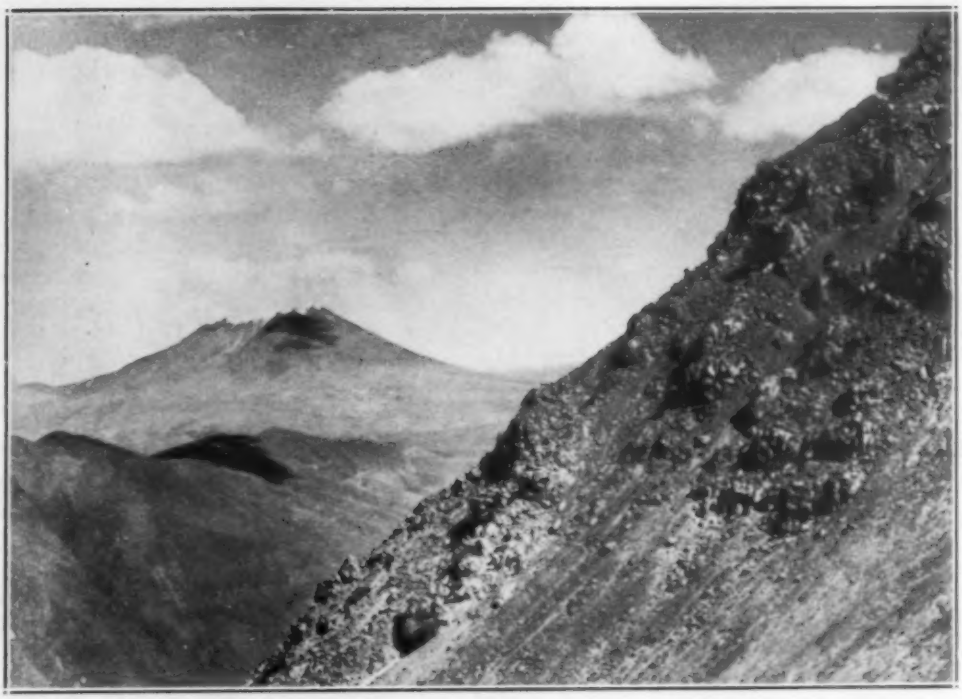
ference of about 1,500 feet in altitude. The large physiological effect of relatively small change in altitude is increasingly noticeable above about 7,000 feet and could be roughly predicted from the known chemical properties of the blood.

Chuquicamata, the world's greatest copper camp, was to be our base of operations in the field. The generosity of the Chile Exploration Company and the friendly helpfulness of the residents there not only facilitated our work but made us comfortably at home. Our work at Chuquicamata was partly concerned with organization of our field laboratory and preparations for the higher stations and partly with experiments and observations. With the generous help of Mr. Arthur Heskett and other officials of the railroad, four railway cars were outfitted for the field work, a sleeping car, a kitchen car and two laboratories. Our gasoline generator provided electric light for the laboratory cars and current for



LLAMA (LEFT) AND VICUNA

AT CHUQUICAMATA (9,200 FEET). DR. HALL IS WEARING A CHILEAN INDIAN SHAWL.



GOING UP ABOUT 17,000 FEET.

NOTE DETRITUS OF SEVERE FROST ACTION BUT ABSENCE OF EROSION.

the centrifuge. Four servants and helpers were engaged, who were not dismayed at the prospect of "puna, carne duro y mucho frio" (mountain sickness, coarse food and great cold).

The altitude has little outward effect on the life of the 18,000 inhabitants of Chuquicamata. In addition to breathlessness, newcomers may suffer from mild headache for a day or two and may be restless at night, but these effects soon disappear. Football and boxing are enthusiastically played by the workers, while golf, and to a smaller extent volley ball and tennis, are popular with the staff, which is largely composed of North Americans. Both groups frequently hold dances where the tempo is by no means slow nor does fatigue cause them to stop early.

Both workmen and staff form a permanent population at Chuquicamata, and many people there have records of residence as long as twenty years and more. The staff members return home for several months every few years and make fairly frequent visits to the coastal towns of Antofagasta and Tocopilla. The workmen, however, rarely make visits to the lowland. In the case of the staff, the loneliness and desolation of the surroundings and lack of diversion are probably more compelling reasons than altitude for return to the lowland, *i.e.*, the reasons are more psychological than physiological, but it is not improbable that the undercurrent physiological effects have some bearing on the general state of mind.

At this altitude acclimatization is very rapid, most of the adjustment in our group being accomplished in less than a week, but we could see further improvement for several weeks. Even with com-

plete acclimatization, however, it must be realized that in some respects man is definitely physically inferior at this altitude as compared with sea level. The frequent sight of workmen running home a mile or more is impressive chiefly because it is surprising that they retain so large a fraction of their sea-level ability. As would be predicted from physiological considerations, the effects of altitude become prominent in work of great intensity such as can be maintained only for a period of some minutes; moderately hard work is little more tiring than at sea level. Mental and sensory functions are not affected.[8]

Aside from the expected increase in concentration of red cells in the blood we could find no characteristic results of long residence at Chuquicamata. The large hospital, which receives cases from many miles around, is admirably run and keeps excellent records. From consultations with the chief of the hospital, Dr. Pablo Schlack, and his assistants it was learned that the course and incidence of disease is generally similar to the lowland. Not only is the incidence of pneumonia not abnormal, but recovery does not seem to be hindered by the altitude. Infant mortality is lower than in many lowland communities in Chile, and maternal mortality is not high by North American and European standards.

At Chuquicamata we began to assemble our small zoo of experimental animals which, under the watchful eye of Dr. Hall, accompanied us later to all except the highest camp. As representative lowland animals we took rabbits, ducks and a sheep; for highland animals we had a llama, a pair of viscachas (*Lagostomus* species, large rodents native to the Andes), and the pet of the expedition, a tame, half-grown vicuña (*Lama huanachus glama*). Later we added a pair of Andean geese ("huallatas," *Chloëphaga melanoptera*) and a Bolivian ostrich or rhea (*Rhea americana*). These animals were housed in a special section of one of the laboratory cars when our train left Chuquicamata for Ollagüe on June 5.

Ollagüe (12,020 feet) is on an arm of the great "puna" (high plain) of southwestern Bolivia. Dozens of peaks, 18,000 to more than 20,000 feet high, cut off the horizon in most directions. Close to the tops of many of these mountains in this volcanic region occur great deposits of native sulfur, the mining of which carries man to greater heights of permanent occupation than anywhere else in the world.

Ollagüe is the center of life for many miles around. Its 900 inhabitants are mostly of mixed Quechua Indian stock who, like their ancestors for untold generations, have rarely or ever been below 10,000 feet. The Quechuas, with the anthropologically distinct Aymaras, who are rare in this region, divided the highlands between them long before the Incas came. Many local customs are still preserved, but the influence of the energetic Chileans, who are increasing in numbers in this region, is gradually making itself felt.

The broad and glaring plaza of Ollagüe is the meeting ground for the rival football teams from the nearby mines. The games are not very fast by sea-level standards, but offer remarkable testimony to the power of acclimatization to counteract the paralyzing effect of the thin air. One is reminded of the fact that polo apparently originated in a not dissimilar environment in Baltistan and Ladakh, where every village still has its polo field.

At Ollagüe Drs. Talbott and Barron discovered two cases of "Monge's disease," a chronic form of mountain sickness recently described by Dr. Carlos Monge of Lima, in which the sufferers, chiefly adult natives of the highlands, complain of a chronic throat irritation, loss of appetite, nausea, muscular weak-

[8] The detailed psychological findings for all the stations are being published in the appropriate technical journals by Dr. Ross A. McFarland.

'QUILCHA MINE CAMP (17,400 FEET).

LIVING QUARTERS OF THE WORKMEN SHOW AS A LINE IN THE LOWER FOREGROUND. THE LIGHT COLOR IS MOSTLY SULFUR ORE DUST, MIXED WITH OLD SNOW IN THE UPPER THIRD OF THE PHOTOGRAPH. THE MAIN PEAKS ARE TO THE LEFT OF THE PICTURE.

ness and pain, dyspnea (panting) and feeling of suffocation and anxiety. Such persons, like those suffering from acute or transient mountain sickness, quickly lose all symptoms when brought to a low altitude, but at high altitude the condition is refractory to medical treatment.

At Ollagüe for a day or two several members of the expedition had mild cases of mountain sickness and others slept poorly, but laboratory work went on. Within a week seven members had climbed to more than 18,000 feet on Volcano Ollagüe and two had climbed to the top of Aucanquilcha (20,280 feet), where we planned to establish an experimental station later.

From Ollagüe a spur line of the railroad winds northward for fifty miles up rolling hills to Collahuasi and the Poderosa and Miño Grande mines, attaining an altitude of 15,650 feet at the summit, Punta Alta.[9] Before they were shut down in 1930 these mines supported a population of several thousand people, which probably represented the largest population at such extreme altitude of which there is record. The usable railroad now ends 200 feet lower at Montt, where the expedition remained for two weeks. This is considerably above the highest point at which detailed observations had been made previously and our time was occupied by intensive experimentation.

Again, several members of the expedition were incapacitated for a short time, but each day brought increasing acclimatization. A Sunday excursion with Messrs. Packard and Bell, the English managers of the property, to the source of the Rio Loa at 12,000 feet, was a real holiday, including a swimming party in a warm pool and harmonica concerts uninterrupted by breathlessness.[10]

[9] Slightly higher than Ticlio, Peru, often considered the highest point in the world to which rail has been laid.

[10] Later, at 20,140 feet on Auconquilcha, Matthews entertained the writer on the harmonica for six nights, eloquent testimony to the power of acclimatization.

At Collahuasi Mr. Edwards arranged a hill climb in which three subjects exerted themselves to the absolute limit. He had been studying lactic acid, which normally accumulates in the blood when one gets out of breath from hard work. Many investigators had thought that the lactic acid causes or at least is necessarily related to the exhaustion of intense exercise. From our experiments on the stationary bicycle, however, we had been surprised to find that with increasing altitude there was a relatively decreased production of lactic acid in exhausting work. These results were confirmed by the hill climb and by later climbing and bicycle experiments at still higher altitudes.[11]

The expedition train returned to Ollagüe, where it was to be the base laboratory for the highest work. The sketch of Mount Aucanquilcha shows the relations between Ollagüe, the 'Quilcha Mine camp, the mine and our highest camp. From Ollagüe the Carrasco Mine Camp, 'Quilcha, can be reached by motor cars (with special low gears and cut-down cylinder heads) in an hour and a half. About 150 people live here at 17,500 feet, with the distinction of forming what is probably the highest permanent community in the world, and the camp merits special attention for this reason. The camp stands on a shelf several hundred yards wide on the northeast side of the mountain directly below the mine (Fig. 1). Water is carted up from lower springs.

The mine itself is at the extreme height of nearly 19,000 feet. The ore is brought down by a series of aerial tramways. The miners, however, go up to and down from the mine on foot, the climb being made in about an hour and a half and the return in about 25 minutes. Work is continuous throughout the year, except for brief periods during storms.

[11] The results and discussion are being published by Mr. H. T. Edwards in the *American Journal of Physiology*.

MULE-BACK AT 'QUILCHA.

MULES CAN BE RIDDEN TO 18,800 FEET.

Mining materials are carried up by mules (horses can not stand the altitude), but large pieces of equipment are carried on the shoulders of a dozen or more men who require an entire day for one trip of this sort. In general, the miners regulate their own working hours. Usually they start the climb to the mine shortly before 8:00 A.M. and start down again towards 4:00 P.M.; the luncheon pause is brief. The work at the mine is mainly rather heavy manual labor, but the tempo is slow. The experienced miners know their capacity and it is rare to see acute shortness of breath.

A number of the miners have their families also at 'Quilcha. Stone huts provide primitive but effective shelter against the bitter wind and cold. The life, though hard, is not unbearable, and many people have been there for years. The labor turnover is high but not so high as casual inspection of the figures would indicate. Many of the miners quit work every six months or so but return from below (some of them go down to sea level) when their savings are exhausted. Among newcomers to the mine there is a very high turnover, because many of these men (most of them long accustomed to life at 12,000 feet) quickly discover that they can not become adjusted to the higher altitude. High wages bring a continual stream of job-seekers. The main population at 'Quilcha represents a group highly selected with respect to their ability to withstand the altitude.

We were surprised to learn that many of the best "old-timers" at the mine are Chileans, born and bred near sea level. The Andean peoples are by no means immune to "soroche" or "puna," and many lowland people are able to compete with them successfully in their own habitat after some months of acclimatization.

Diversions are few, a visit to Ollagüe being by long odds the most welcome. At 'Quilcha football is played half-

heartedly—think of it when you are climbing Mt. Blanc, *only* 15,800 feet!—but the real games are held at Ollagüe. The chief solace seems to be coca, brought in from the Bolivian lowlands. The dried leaves are chewed with bits of a "biscuit" made from alkaline wood ashes. The efficiency of the extraction of cocaine from this alkaline mixture must be rather high and the use of as much as a pound of dried leaves a day—which is not uncommon—represents a very considerable amount of the alkaloid. The habitués (and abstainers are rare) chew these leaves during most of the day and ascribe to them their ability to withstand not only altitude but cold, hunger and fatigue as well. We regretted that we had no place in our program for a study of this interesting habit, which is common to much of the Central Andes.

Through the great generosity of Señores Don Juan and Don Hipolito Carrasco we were provided with not only transportation service between Ollagüe and the mine camp but also the use of most of the administration building at 'Quilcha.[12]

At first only a small group from the expedition with a limited amount of apparatus moved up to 'Quilcha, blood samples, etc., being sent down to Ollagüe. We found, however, that laboratory work could be done efficiently at 17,500 feet, and eventually the entire expedition, experimental animals and all, was settled at 'Quilcha.

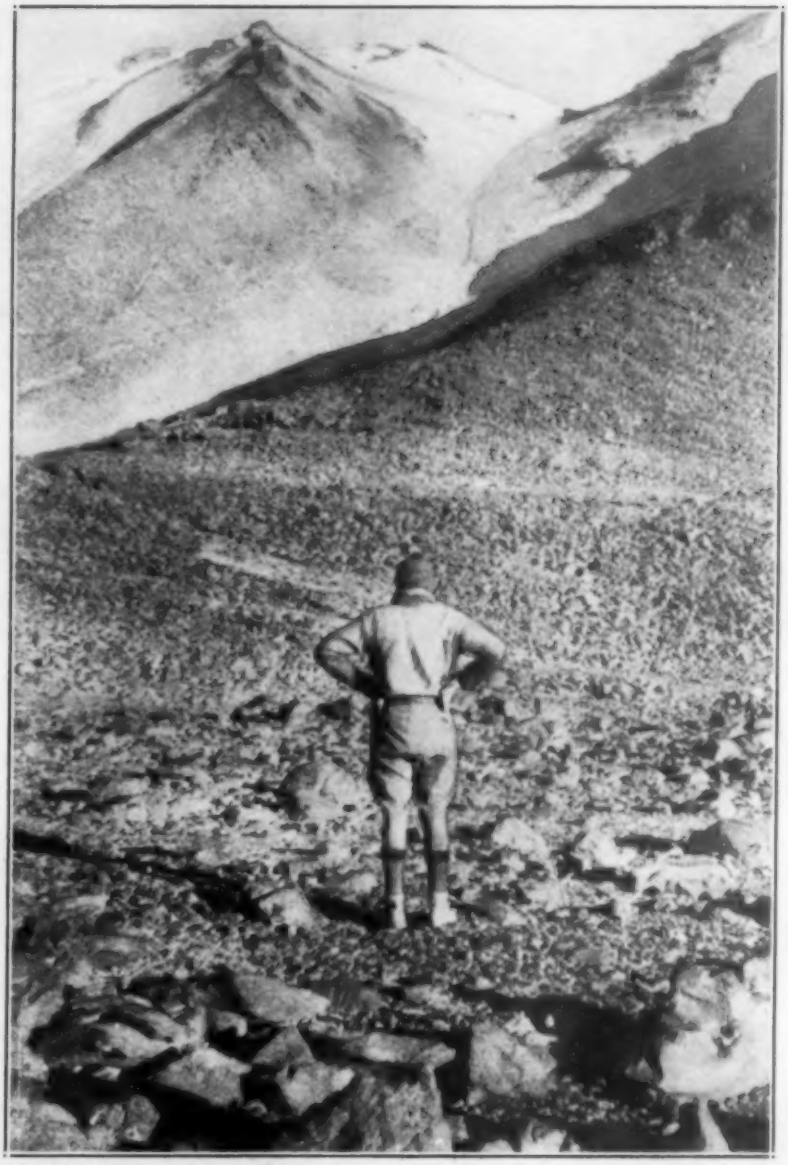
Our activities frequently amused the miners, but they were very friendly and cooperative. We always had plenty of volunteer subjects from which to take arterial blood and samples of the air in the bottom of the lungs (alveolar air). When we told them we were going to establish a camp at the top of the mountain, above 20,000 feet, they said with conviction that neither we nor any one else could sleep there, let alone live there for a week, but they cheerfully shouldered packs and followed us up to help make our camp.

As eventually established, the route to the top was both easy and safe, the gradient averaging about 35 per cent. and nowhere exceeding 55 per cent. The climb up was a long hard pull, but blood and gas samples could be sent down to 'Quilcha in only an hour or so. The camp consisted of two glacier tents and a snow cavern about 7 feet in diameter and 5 feet deep, roofed with tent poles covered with blankets topped by snow. The snow cavern was the principal living quarters and, with its twisted tunnel entrance, effectually shut out the wind which occasionally threatened to destroy the tents. This camp was occupied by successive pairs of men for periods of a week, two days, two days, and a day and a night. From all these men samples of arterial blood and alveolar air were obtained and satisfactorily analyzed.

It must be remembered that, on the whole, our party was very well acclimatized and that for the top camp we selected those who seemed best adjusted. Even so, we were surprised that life at over 20,000 feet can be maintained so long with so little serious difficulty. One member became gravely ill with "soroche" after a day at the top (he recovered quickly when brought down to 12,000 feet), but otherwise it was by no means a case of existing by sheer effort of will in a state of physical and mental torpor.

Reports of mountaineers who were unable to eat or sleep at this altitude had not led us to expect the good appetites and restful sleep we experienced. However, the total food intake was definitely subnormal, as was the time spent in actual sleep. Because of the cold much time was spent in our sleeping bags, but the long waking hours passed restfully.

[12] We have not forgotten the hospitality of Mrs. Carrasco at Ollagüe, who kept us from forgetting the amenities of civilization.

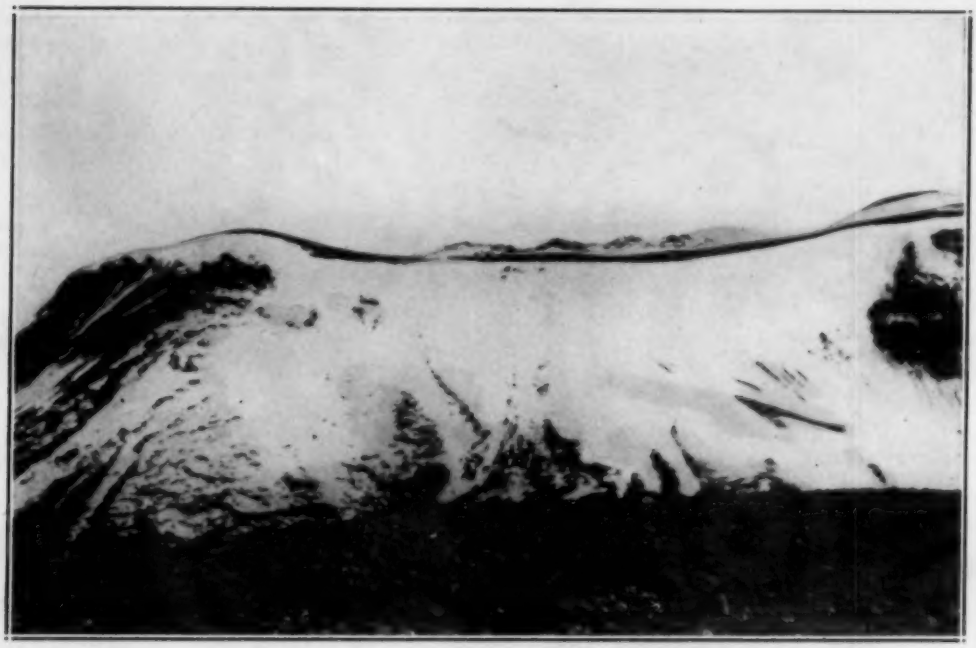


CHOOSING THE WAY UP.

THE PEAK IN THE TOP CENTER IS A SHOULDER OF THE HIGHEST SUMMIT OF AUCANQUILCHA.

Smoking was impossible when doing any physical labor, but otherwise was as enjoyable as at ordinary altitudes and our tobacco consumption did not decrease.

The continuous cold was trying and the occasional high winds intensified this discomfort. Preparing food consumed much time and energy—thawing tinned foods in water boiling at 175° Fahrenheit is slow. Cold hands and feet persisted except when walking or after some time in bed. The body, and especially the extremities, is more sensitive to cold at high altitude, and at our high camp (in



CENTRAL SUMMIT RIDGE OF AUCANQUILCHA

AS SEEN FROM THE SOUTHEAST PEAK (NEAREST OLLAGÜE, SEE PHOTOGRAPH ON PAGE 290). THE ROUNDED SUMMIT AT THE EXTREME RIGHT IS THE HIGHEST POINT ON THE MOUNTAIN (20,280 FEET). THE HIGHEST CAMP (PUNTA DE CERRO) IS ON THE SNOW FIELD JUST BELOW THIS SUMMIT BUT IS NOT VISIBLE AT THIS DISTANCE. BETWEEN THE PHOTOGRAPHER AND THE CENTRAL SUMMIT RIDGE THERE IS A SNOW-CLAD VALLEY 3,000 FEET DEEP.

the South American winter) we experienced true arctic cold. Daily maximum temperatures were quite constant, averaging 10° F. Minimum temperatures were more variable, ranging to as high as −7° F. and averaging about −20° F. The lowest temperature during our stay at the top was −45° F.

All members of the expedition lost weight during the stay above Chuquicamata in spite of abundant, varied and generally not badly cooked food. The average weight loss was close to 15 pounds per man, and one member lost 26 pounds. A part of this, however, was merely dehydration, and most of the lost weight was regained in a few weeks at more moderate altitudes. Really fat people are rare at high altitude, though the "chunky" type is common.

On going down to sea level there is little or no feeling of exhilaration, only a sense of well-being, enormous appetite and readiness to sleep twelve hours on end. There is no feeling of "treading on air" or of limitless power. The idea that the person recently from high altitude is a much better man at sea level than the sea-level stay-at-home is probably entirely erroneous. It must be remembered that the adjustment to high altitude involves much more than mere increased efficiency in breathing and in transporting oxygen. For example, the respiratory centers in the brain which control the rate of breathing may have altered in sensitivity and the new sensitivity, nicely adjusted for the high altitude condition, may be ill-suited for sea level. Again, we found that the carbon-

dioxide capacity of the blood is greatly diminished in the acclimatized person at high altitude. This is an admirable arrangement at high altitude where carbon dioxide elimination tends to outrun the ability of the body to take up oxygen, but it would be rather a hindrance at sea level.

The detailed results of the expedition are being published in the technical journals by the various members of the expedition. It is not intended to attempt a résumé here; as a matter of fact, at the time of writing not all the data have been analyzed fully and it will be some time before the last of the papers resulting from the expedition are published. However, a few high lights may be given in addition to the points already noted.

As a preliminary, it is necessary to review some of the known facts about respiration at ordinary altitudes. When air is drawn into the lungs it comes into close contact with the blood. The oxygen pressure in the air is considerably more than in the venous blood and the blood tends to take up oxygen from it. The arterial blood leaving the lungs has an oxygen pressure in it not much less than the oxygen pressure in the air in the bottom of the lungs (alveolar air). Only a part of the oxygen in the lungs is absorbed, however, and the amount of oxygen taken up by the blood is dependent upon the oxygen pressure and the amount of hemoglobin in the blood. This hemoglobin, the colored matter of the red cells, unites with oxygen and so enables normal blood to carry about 50 times as much oxygen as ordinary salt solution. But at sea-level air pressure the hemoglobin becomes "saturated" and will take up no more oxygen. Accordingly, at or near sea level, normal arterial blood from the lungs is always nearly (95 to 96 per cent.) "saturated" with oxygen and carries its full quota to the tissues of the body. At low air (and oxygen) pressure, however, the hemoglobin will be only partly saturated and the blood will be deficient in oxygen.

The late Professor J. S. Haldane had suggested that at high altitude the cells of the lungs somehow "learn" to secrete oxygen into the blood. According to this theory, the pressure of oxygen in the arterial blood of an acclimatized man at high altitude should be higher than the pressure of oxygen in the air at the bottom of the lungs. This theory had been denied by many, notably Professor August Krogh in Denmark and Professor Sir Joseph Barcroft in England, but the Oxford school could always retort that the subjects were either not acclimatized or that the experiments were at too low an altitude.

We made very careful measurements of the oxygen pressure at the same time in the arterial blood and in the lungs, both in ourselves and in the residents at all altitudes. In no case did we find any support for Haldane's theory; the same process of simple diffusion in the lungs as at sea level could account for all our results.[13]

The acidity of the blood does not affect its total oxygen capacity, that is to say, the total amount of oxygen taken up by the blood is not affected by the acidity so long as the oxygen pressure is high enough, as it always is at sea level. However, when the oxygen pressure is low, the blood is more nearly saturated if the blood is relatively alkaline.[14] Sir Joseph Barcroft pointed out that at high altitude the blood should be more alkaline than at sea level and so should be less unsaturated than would be predicted otherwise. The argument for this change in alkalinity is, in brief: The acidity of the blood is controlled chiefly by the

[13] These results were published in the *American Journal of Physiology* by Drs. D. B. Dill, E. H. Christensen and H. T. Edwards.

[14] Eating or drinking alkaline "tonics" or so-called "alkalizing" foods will give little effect and less benefit in this respect.

PREPARING THE HIGHEST CAMP.

MATTHEWS RESTS A BIT.

THE HIGHEST CAMP COMPLETED.

THE ENTRANCE TO THE SNOW CAVERN AT THE LEFT OF THE TENTS IS MARKED BY THE ICE AXE. SOUTHEAST PEAK IN THE BACKGROUND.

amount of carbonic acid (or carbon dioxide) in the blood. This carbonic acid is being produced all the time by the body cells and is being removed in the lungs where a part of the carbon dioxide diffuses into the air. When breathing is increased, more air circulates in the lungs, a larger fraction of the carbon dioxide is removed and the blood becomes less acid (or more alkaline). Now at high altitude, the breathing is much increased, and since the production of carbon dioxide is the same as at sea level (*i.e.*, the metabolism is not changed), the carbon dioxide ought to be removed more completely in the lungs and the blood should be more alkaline.

DR. TALBOTT TAKES A SAMPLE OF ARTERIAL BLOOD FROM THE WRITER AT 20,140 FEET.

We found that, to some extent, this is true. Our own blood became increasingly alkaline up to 12,000 feet, but above that level there was either no further change or a return to more normal acidity.[15] However, none of the changes in acidity were large except in several men who acclimatized badly. In the miners the blood acidity was the same as at sea level. The reason is not that the basis of Barcroft's argument is wrong, but that, at high altitude, the blood changes in other ways so that only a small amount of carbon dioxide is enough to make the blood relatively acid. This is another example of the efficiency of the body in tending to keep the acidity of the blood constant under all conditions.

Barcroft had further proposed that the fundamental affinity (at constant acidity) of hemoglobin for oxygen is increased in the acclimatized man. In other words, though the total amount of oxygen which hemoglobin can hold at *high* pressure is always the same, Barcroft suggested that the hemoglobin from the mountain dweller may unite with oxygen more readily than sea-level hemoglobin.

We measured this fundamental affinity

[15] The studies on blood acidity are being published by Dr. W. H. Forbes, the writer and Dr. F. G. Hall.

VIEW FROM THE HIGHEST CAMP.

of hemoglobin for oxygen in all our bloods at sea level and at every station, including the top camp at 20,000 feet. In addition, we made similar measurements with the blood of three residents at Ollagüe and of eight of the miners at 'Quilcha. All these results disagreed with Barcroft's theory. At the lower stations, Chuquicamata and Ollagüe, there was no change in the basic affinity of our hemoglobin for oxygen and the affinity was apparently normal in the miners. At the higher stations there was a slight *decrease* in the basic affinity of our hemoglobin for oxygen. This was partly offset by the slight alkalinity of our blood, so that the net result was to make our blood have about the same amount of oxygen as would be predicted for blood brought directly from sea level.[16]

The consideration which impelled both Haldane and Barcroft to advance their theories was the realization that otherwise it would be necessary to admit that at high altitude the arterial blood, even of acclimatized men, must be greatly unsaturated with oxygen. And hospital experience had shown that when the arterial blood is greatly unsaturated the patient is very sick indeed. However, we found that both our arterial blood and that of the miners *was* greatly unsaturated and to about the same extent in both groups at 'Quilcha. At the highest camp our arterial blood averaged only 66 per cent. in arterial saturation, yet as already recorded, we felt well, able to do moderately hard work and even, or so we believed, able to think straight.

Many mountain-climbing parties previously had found that the concentration of red cells in the blood is increased at high altitude and that the increase is roughly proportional to the altitude. This thickening of the blood was very evident in our party, and we found, in addition, that the oxygen capacity (the total amount of oxygen held by the blood when fully saturated) increases in precisely the same proportion as the number of red cells.[17] The oxygen capacity of the miners' bloods at 'Quilcha was more than 50 per cent. above the average for sea level. Our own bloods increased on the average 25 per cent. These high oxygen capacities would be enough to overcome the handicap of our low arterial saturations if all the oxygen could be given up to the tissues. However, the venous blood always carries some oxygen back to the lungs and the tissues ordinarily suffer if they have to use oxygen at very low pressure. It may be that much of the acclimatization to high altitude consists in the habituation of the tissues to live, not on less oxygen, but on oxygen at a lower pressure.

In any case, the oxygen delivered to the tissues depends on the rate at which the blood is pumped around the body as well as on the amount of oxygen in the blood at any one time. The attempt of the body to make some adjustment along these lines accounts for the fact that when a sudden ascent is made the heart rate is increased even in resting people. In the acclimatized person, however, the simple expedient of pumping faster is not resorted to except in response to work. Our own pulse rates in rest did not increase except in those suffering from mountain sickness, and in several of those best acclimatized the pulse rate was actually slower than at sea level. The miners at 'Quilcha had considerably slower pulses than comparable men at sea level. This is fortunate, since at high altitude the heart is already under some strain from the job of pumping unusually thick and viscous blood. However, at high altitude, relatively mild exercise brings about an increase in pulse rate like heavy work at sea level, and

[16] *Cf.* Keys, Hall and Barron, in the *American Journal of Physiology*, 115: 292–307.

[17] The studies on blood morphology are being published by Dr. J. H. Talbott.

most of the long-time residents at 'Quilcha show some degree of heart dilatation similar to that seen in athletes.[18]

The appetite is apt to be capricious at high altitude, but sweet foods, chocolate and jams are often relished. The blood sugar sometimes may be a little high and dextrose seems to be rapidly absorbed; this is in agreement with the subjective observations of mountaineers and gives point to the decision of the present Everest Expedition to take along a supply of dextrose.[19]

The study of the high altitude animals was of much interest, though we could only scratch the surface of this fascinating field.[20] These animals, llama, vicuña, etc., did not have a very great amount of hemoglobin in their bloods, but we found that there is an unusually great affinity of their hemoglobins for oxygen, and the oxygen saturation of their arterial bloods generally surpassed our own. On the other hand, we were generally superior to lowland animals like the sheep and rabbit. Barcroft's theory works when comparing different types of animals if not for explaining acclimatization in man.

One may ask what are the limits to life at high altitude. The highest peak yet climbed is Kamet (in India, 25,400 feet), but at least six men have surpassed 28,000 feet on Everest. Apart from the sheer climbing difficulties, it is probable that Everest could be climbed without extra oxygen, but that altitude (29,140 feet) must be close to the absolute limit. Birds have been seen in the Himalayas as high as 26,000 feet but, in general, animals do not go above the level where food may be found except when they are pursued. The highest cultivated land seems to be 15,200 feet at Korzok in Little Tibet, but sparse grazing may extend up another thousand feet. Lichens are found higher than any other types of plants and we found many small patches at 20,000 feet.

The limits for semi-permanent human populations may be gauged from the 'Quilcha camp. At 'Quilcha the attempt was made to put the camp closer to the mine. For more than six months a camp was maintained at 18,500 feet, but even these highly selected people were beaten down. They had suffered from increasingly persistent headache and sleeplessness, and now they make the exhausting climb to the mine daily with no thought of moving their camp higher.

It must be clear that there is still much to be done on the problems of life at high altitude. Many false trails have been eliminated and much positive knowledge has been gained, but such questions as the rôle of the body tissues and the activity of the respiratory centers in the brain are almost entirely unanswered. Again, we know next to nothing about the distribution and rate of flow of the blood in various parts of the body. We can say that generally the man best fitted to go to high altitude is strong, active and young, with a large lung capacity and a rather slow resting pulse rate at sea level. But we can not say precisely what determines that one man will be mountain-sick, while his fellow literally feels "on top of the world." Many of us will not rest content until we can return better answers to these questions.

[18] The heart studies are being published by Mr. Bryan H. C. Matthews.

[19] The studies on sugar metabolism are being published by Dr. W. H. Forbes.

[20] These results are being published by Dr. F. G. Hall and Dr. D. B. Dill.

IN QUEST OF GORILLAS

By Dr. WILLIAM KING GREGORY

CURATOR OF COMPARATIVE ANATOMY AND OF ICHTHYOLOGY, AMERICAN MUSEUM OF NATURAL HISTORY; PROFESSOR OF PALEONTOLOGY, COLUMBIA UNIVERSITY

XII. HUNTING GORILLAS IN WEST AFRICA[1]

By H. C. RAVEN

ASSOCIATE CURATOR IN THE AMERICAN MUSEUM OF NATURAL HISTORY; LECTURER, COLUMBIA UNIVERSITY

FROM the village of Ozoum we looked over to the northwest at some rather steep hills, the upper slope of which seemed to be covered with heavy forests, while on the lower slopes there were alternate patches of second growth and native gardens.

Probably no people are more expert in sending and receiving messages by drum than are these West Africans, and drums are to be heard at almost any time of the day or night. After considerable discussion with the head-man of Ozoum, Martin Atangana, it was decided that he should send out word to all the neighboring hamlets, of which there were a number generally distributed on the lower

[1] In the previous section I mentioned Raven's indefatigable hunt for gorillas in the forests near Ozoum in the Yaoundé region and later at Vimeli and other localities, all in the French Cameroons. Raven's own story of his adventures, based upon his very full diary and subsequently dictated records, will now be given. —W. K. GREGORY.

SOLID COMFORT--FOR AFRICANS.

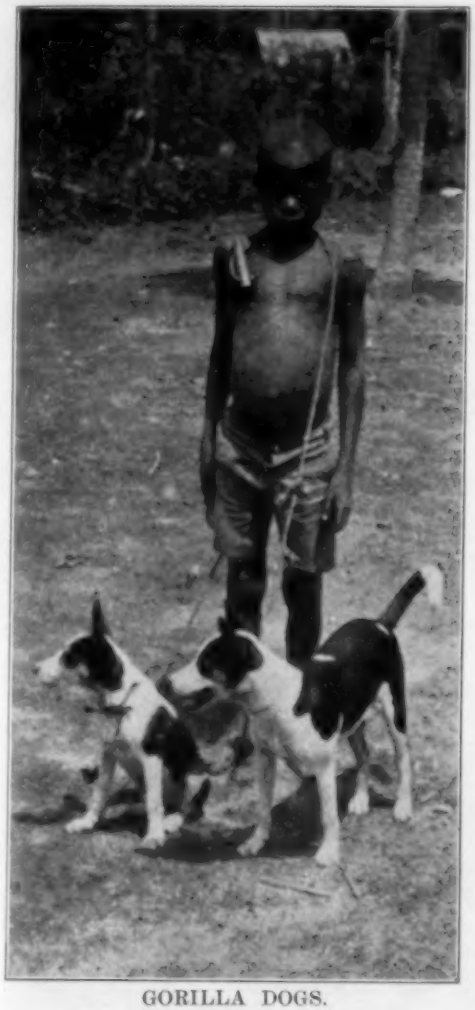
slopes of the range of hills to the north and northeast of his village. He was to send word that should gorillas be heard or seen he was to be notified immediately, because in his village was a white man who wished to shoot them and who would come to hunt them as soon as they were reported.

On the morning of November 16, 1929, by which time I had already begun to feel impatient, a drum reported the presence of gorillas at a place called Ongke, several miles away. With Martin and a few carriers I left Ozoum at 10 A. M. When we reached Ongke I was told that the gorillas were right over among the plantains. I stepped over the bamboo fence and walked down the slippery incline, for the ground was red clay and it had been raining. I followed the path down toward a brook. On my left there was a dense second growth, while on the right there was high grass and plantains. I had with me one man.

As we walked quietly along the path for 50 yards we could hear not a sound, but suddenly there was the distinct and characteristic noise made by a gorilla when breaking down a plantain or banana plant, a sound very much like that which might be made by tearing to pieces a head of cabbage. We then went on very quietly and could hear the same sort of noise in the second growth on the left and among the plantains and grass on the right, while in the mud of the path we clearly saw the knuckle-marks made by gorillas.

We waited a few minutes, hoping that the animals might cross the path one way or the other; though when they did not we started after them and found

A SUNDAY DISH—DRIED BEETLE LARVAE.

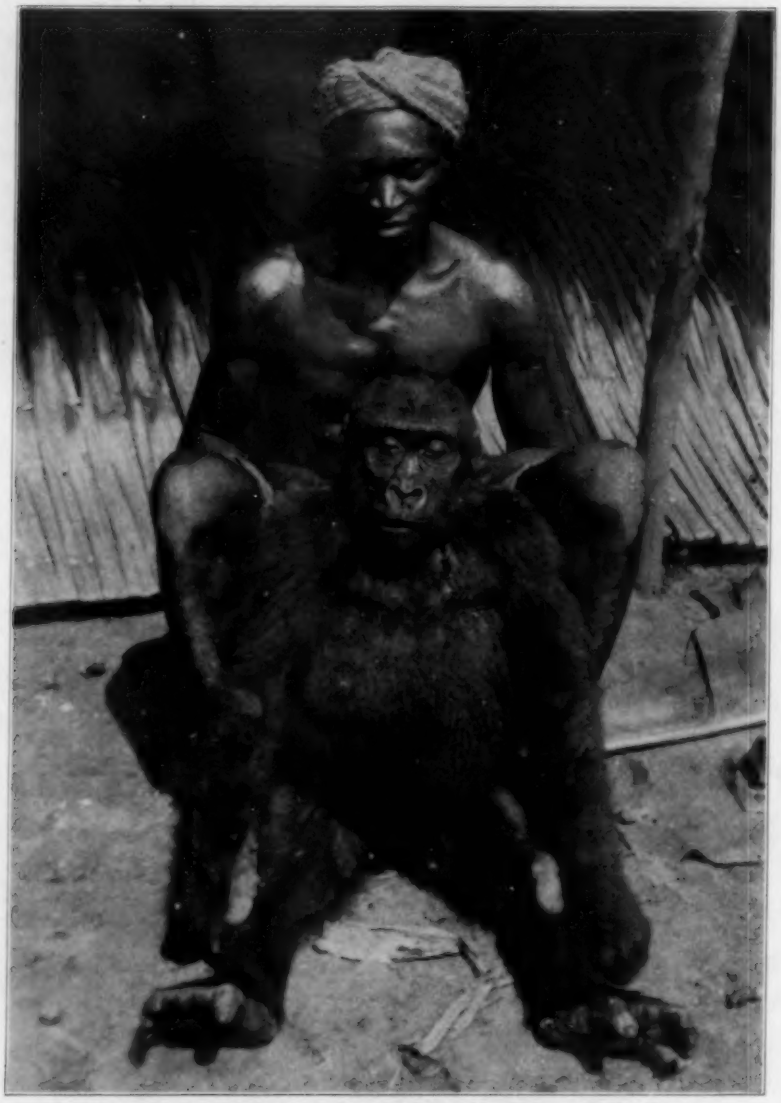


GORILLA DOGS.

that they were moving away. I then thought it would be easier and less likely to scare them away if we walked in the brook. However we had gone only a few yards when we came to a place where gorillas had crossed the brook. We thought they could not be far because there had been no signs of alarm, such as slapping the hands or beating the breast or both.

We found a little trail on the far side of the brook, leading up hill. Most of the gorillas seemed to have gone to the left of this path, but we could hear one ahead and to the right. We got quite close to him because the vegetation was very wet and we were able to go quietly. However, he discovered our presence and we knew that we were discovered, for he remained absolutely still for several minutes. Then when we tried to advance he screamed and rushed at us, though we could not seem him. He

AFRAMOMUM PLANT.



YOUNG GORILLA THAT HAD BEEN CAUGHT IN A SNARE.

stopped perhaps fifteen yards from us and then went quietly away.

We followed as quietly as we could. Twice more he stamped and came forward, then continued for more than a mile, while we followed. One time I caught a glimpse of him in the gloom under the enormous lianas near the trunks of big trees.

Perhaps I should have fired, but I waited, hoping to get a better shot. But we saw no more of him. He went away more quickly than we could follow, and we lost the trail when he reached a rocky place at the headwaters of a tiny brook. He had led us right over into primeval forest and then had eluded us, which he could not have done in the grass and

second growth of the type near the clearings.

We waded around all the rest of the day, trying to catch up again with this or another group of gorillas but without success. I returned to the clearing and learned that some of that same group of gorillas had been eating sugar-cane while we were following the single one. I had a look at the cane, and were it not for the great strength exhibited in breaking the canes and the fact that the canes had been torn open by strong teeth where a man would have used a knife, one might have thought that it had been eaten by humans, for the fibrous inner parts had been chewed to extract the juice and the pressed parts had then dropped on the ground. Some of the strong-jawed blacks perhaps could have torn open sugar-cane with their teeth, but when the gorillas do it the marks of the big canine teeth are distinctive.

Having failed to get the gorilla, I decided to sleep at Ongke, so sent a boy back to Ozoum to get my bed-roll. As he had not returned when I was ready to go to bed, I lay down to sleep in a little hut, the head-man Martin on one side of the fire and I on the other. We slept on beds consisting of a series of poles like a corrugated iron roof, made of the stems of palm leaves, hard and smooth and round. These beds have raised cross-pieces at the head and foot.

ON THE WAY TO DJAPOSTEN.

He slept with apparent comfort, but I was not accustomed to that kind of head-rest or foot-rest. The smoke was not so bad when one was lying down, but it was dense, for the door was closed and there was no chimney. It had to seep out through the palm mats, which had been blackened by it until they were like lacquer.

The following day we took a half-dozen natives and went over a consider-

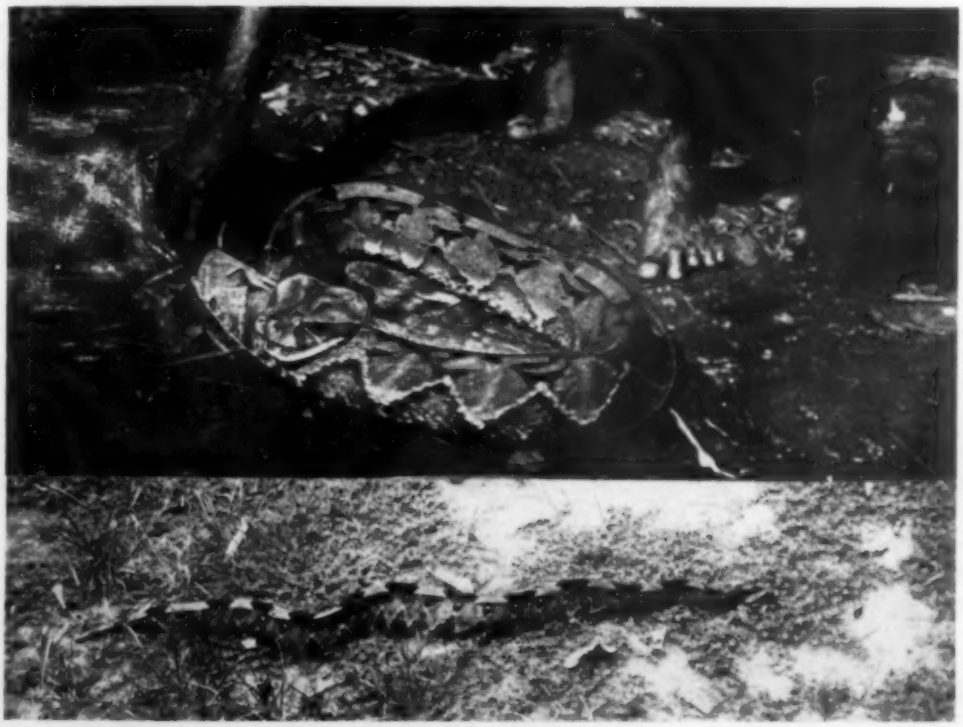
able area but found no fresh traces of gorilla. That night we ended up at a little village between Ongke and Ozoum. Here the head-man's name was Benedict, which, according to the custom, was also the name of the village. As I had not expected to be away from Ozoum over night, I had brought no food, but it was only about an hour's walk to Ozoum and I sent a boy to bring food from there.

TYPICAL CAMEROON ROAD.

The food did not come, the natives had already had their vesper service in the little chapel and it was dark. One of the women of Benedict's household brought food for Chief Martin and me. There were boiled yams about the size of a very large potato, a pure white starchy food, good tasting and of firm texture. Then there was a dish entirely new to me that was called *ngwan* and which I liked very much. It consisted of the kernels of squash seeds dried, peeled and ground on stone, then wrapped in leaves and boiled. I was told that there was meat inside the mixture. It had been seasoned with very hot peppers, which was very much to my liking. We were sitting and eating in the dark, but I found a piece of dried meat about as big around as my finger and a couple of inches long in the *ngwan* and asked Martin: "What kind meat live[2] for inside?" He replied: "Dem good chop; white man he chop 'em." After eating two or three more pieces I examined one carefully by the fire and discovered that they were enormous beetle larvae which had been so thoroughly dried as to be almost tasteless except for a kind of smoky flavor. On another night I was delighted to find

[2] In "Pidgin" the verb "live" does duty for the verb "to be."

PLANTAIN EATER.



a AND *b* GABOON VIPER.

AN ANCIENT WEAPON, THE CROSSBOW.

small prawns cooked in the *ngwan*. They were delicious, and many times later I especially asked the natives to prepare squash seeds with prawns.

I returned to Ozoum on the morning of November 19, having spent two days and nights at Benedict hunting with the head-man. I waited again at Ozoum until the morning of the 21st, and having received no reports of gorillas we started out along a different trail toward the north, parallel with the range of hills. Part of the time it was raining. We passed through several little villages.

At one of these we waited while a man supposed to be a very good hunter was sent for. Martin told me that this man had dogs and had hunted gorillas. However, when he finally came in he proved to be an old grizzled Negro with loose skin about his knees and a pair of typical yellow dogs with up-standing ears. To the neck of one was attached a large wooden bell. These dogs, like all true native dogs, never bark while they are hunting, consequently these wooden bells are used to indicate their whereabouts. One of the dogs had his tail cut off, like a fox terrier. I was told that this was to prevent the gorilla from grabbing him by the tail.

Later on the same morning we stopped at Englebird's village, a picturesque place which sloped toward a stream on the west, from which rose steep hills. The rain made each separate hill and ravine stand out in relief most beautifully.

It was late in the afternoon when we arrived high up in these hills in the midst of a forest in which there was a hut in a small clearing. The owner of the hut pointed out to me a place only a few yards away from the hut, where gorillas had come a few days before. At dusk we heard chimpanzees calling in two or three directions, as the mist settled down over the hills. I passed a cold and smoky night in the hut, the natives sleeping on the other side of a partition.

At dawn a couple of natives were sent out in different directions but returned after about two hours saying that they had seen no traces of gorillas. However, a woman who had come in on a trail in another direction, said that gorillas were further down on the western slope of the hills and we started off in that direction.

At nine o'clock we reached a little village called Tjambolo. We stayed here from the 22nd until the morning of the 26th. The day of our arrival we saw many places where gorillas had been feeding, where they had slept and other signs of their presence. On one or two occasions I could even smell them. Here for the first time I heard mention of gorillas talking at night. One morning natives that lived at the base of the hills told us that gorillas had slept very close to their huts, for they heard them talking in the night, and that the gorillas were probably still there or feeding close by. I went down into the swamp, a place where it was very difficult to hunt because the sun in the middle of the day reached through to the vegetation and small sticks cracked when one stepped on them. However, we got very close to the gorillas and could hear them right in among the plantains and bananas. We could hear their stomachs rumble and see the vegetation move on several occasions. I was once or twice within fifteen or twenty yards of gorillas and caught a glimpse of part of an arm and once a perfect view of a whole gorilla but not for long enough to be able to shoot.

After being close to them for perhaps an hour, we maneuvered around across the small stream in the direction in which they appeared to be moving. There two natives were detailed to go back and follow in their tracks very slowly. It was these natives that the gorillas saw, and they bolted across the stream and away into the forest, splitting up into at least three groups. I had glimpses of several gorillas and a perfect

view of one on the bare stream bank, as they were about to cross the stream, but there was no opportunity to fire a decisive shot. We spent the rest of the day trailing them, finally stopping high on the side of the hill where there was a granite ledge that the natives thought the gorillas would surely pass; but as usual, they apparently chose another route. While we were hiding by this ledge a beautiful dark brown *Herpestes*, a large variety of mongoose, came running along within a few feet of me. I went back to the village, shortly before sunset.

During several days at Tjambolo we followed gorillas after finding where they had slept, came close enough to hear them feeding but caught no more than glimpses of them, more often only the waving of grass and vines above. Here they were eating the fruit of the *Aframomum*, elephant-grass, bananas, plantains and some small yellow fruit. In some of the deserted clearings we came upon beautiful orange trees laden with ripe fruit and much fallen fruit on the ground. When we found such a tree we always stopped for a feast on the fruit, but gorillas seemed never to have eaten any of it, although they passed near by. In many cases we saw where they had eaten sugar-cane. *Aframomum* plants were generally distributed in the district, but I seldom saw much fruit in any one spot. In this neighborhood the gorilla feces always contained the black seeds of the *Aframomum*.

Later we followed a forest trail which finally led us to Ongke, and then we went on to Benedict. This completed our circuit of the hills in the vicinity of Ozoum. During this time we saw a place where gorillas had been feeding the day before. They had pushed over banana plants and torn them apart in order to get at the center of the plant. Another time I saw the broken stem of a *Musanga* tree, three inches in diameter, broken off short by a large gorilla apparently in play. This broken tree gave an indication of the gorilla's prodigious strength.

In one place we saw leopard tracks and bits of hair and blood of a small animal which the leopard had overtaken.

On another occasion I saw a native "call" an antelope. He held his nose with his fingers and called "Ngwa-ngwa-nngw-a-a-a," each time louder than before. In a few minutes out bounded a tiny antelope, which I shot. It was about as high as a fox terrier, one of the smallest of living hoofed animals.

While we were on the way back toward Benedict we heard a drum beating. In a minute or so the regular boom-boom-boom-boom signified the end of the message. There was a little pause, and the whole thing was repeated. Martin turned to me and said, "Gorilla live for Benedict," so we hurried on. We left the stream and went up a long incline. As I walked there were more drum messages and Martin said, "They are saying to come quick because the gorillas are there. One gorilla is in Benedict; he is dead. Other gorillas are angry and are staying near by."

We walked as quickly as we could, and when we reached Benedict we found that the place of the gorillas was about three quarters of a mile away. When I arrived at this locality there were a dozen or more natives armed with spears and knives. They said, "The gorillas are right here." They showed me where there was a great deal of elephant grass where one of the natives had had a snare set for wild pigs. His hut was less than 100 yards away. He had just come out of his hut at dawn and was talking with Benedict when they heard a young gorilla scream. The man immediately realized that the young gorilla was in the direction of the pig-snare and perhaps was caught in it. They ran in that direction. There was the little gorilla with the snare drawn tight around his

wrist, of course crying, and a big male gorilla trying to bite through the snare. There were other gorillas near by. As the men drew near and threw a spear, all the gorillas drew away and threatened the natives from the security of the dense underbrush. At first I was told that they had speared a large gorilla, but upon further inquiry I decided that they had thrown spears at him but did not know whether he had been hit. As they approached the little gorilla the natives were very much excited. The gorilla was screaming, and when the man went close it grabbed his spear. He immediately struck it with his machete, cutting off its hand. He said his only thought was to make it keep still so that the others would not come after it. He then struck it on the side of the head and speared it in the chest, killing it instantly. We followed this group of gorillas for some distance and spent until the middle of the afternoon trying to come up with them. However, they were thoroughly alarmed and went a long distance over the hills. Upon returning to Benedict I bought the head of the little gorilla from the natives when they had dismembered it. They ate all parts of the animal, including all the viscera. These people were very keen to get any kind of meat. I believe most of them got a meal of meat no oftener than two or three times a week.

I returned to Ozoum, where I found that Drs. Gregory and McGregor had had an interesting time photographing and making sketches of the natives in and about the village. The head-man Martin wanted us to see a French official in Yaoundé and get him to order a native chief, who he said had many people, to combine and make a drive to hunt the gorillas and drive them up into trees. He said we could easily kill all of them. Of course my object was not mass killing and I would not consent to this. I felt that there must be other districts in the Cameroon where they were more abundant. We therefore decided to return to Yaoundé and go off in some other direction. In Yaoundé one of the French officials told us that he had recently been at Vimeli and that the chef de poste there had a very good *pisteur* and that gorillas were plentiful. We decided to take his advice, hired a camion from an English firm and left for Vimeli with our camp equipment and our three black boys.

The chef de poste, M. Juillot was a very young man who was much interested in animals and had many pets, including dogs, cats, a parrot, a mongoose and best of all a baby gorilla. He very kindly invited us to occupy part of his large house. He called his hunter for me and got me a police boy and three or four porters. His police boy was to supply me with porters in any village I might visit.

We walked a few kilometers back along a motor road toward Yaoundé and for several miles over a trail that went off to the right. We passed some little villages and cocoa plantations as well as native gardens of plantains, bananas, caladium and cassava, corn and peanuts. We then got into country where there was more forest and more rolling ground and where there were a number of little streams and some forested swamps.

On several occasions we heard the swish of branches and hoped that we might come upon gorillas or chimpanzees feeding up in the trees. We would listen and walk quietly, but each time we would either see or hear sounds which we knew were caused by monkeys, not gorillas nor chimpanzees. We saw three or four varieties of monkeys, all of the genus *Lasiopyga* and all of them very beautiful.

In one of the swamps crossed by the

trail we saw fresh footprints and knuckle-marks of the gorillas, which we followed. We followed gorillas by sound for two or three hours and then got into a patch of forest where the ground was drier and harder than in the lowlands and where the trailing was much more difficult. We heard grunts and saw the motion of the vegetation and would go stealthily forward; then over in quite another direction we would hear more sounds.

The hunter that I had with me was a wiry, hard-faced, black Negro, who seemed to take delight in ordering the two other Negroes about, gruffly telling them to scout here or there, right or left. It seemed to annoy these native trackers very much that we could not keep on the trail of the gorilla, but this was very difficult to do in the middle of the day when the ground had got drier and we could not see the footprints and knuckle-marks, but only an occasional broken twig. On several occasions we heard noises in different directions.

After shifting back and forth several times we heard a noise close by and were going stealthily, expecting to meet gorillas at any moment. Suddenly a hunter ahead turned and said, "Schwein!" and asked me if I wished to shoot one. I replied that I did not want to shoot and he then begged to be allowed to do so. This native hunter had an old French rifle. I said, "All right. Go ahead!" and he carefully inspected his rifle. There were several red river hogs (*Potamachoerus*) in the group, beautiful animals with long tesselated ears and quite long tails. Out of curiosity I followed just behind the boy as he went up-wind to shoot. He made use of a fallen tree as cover. There were perhaps a dozen animals, mostly just standing still, switching their tails and flapping their ears. The boy went up until he was within less than fifteen yards of the nearest animal, which was a big one. Then he deliberately put up his rifle as he was kneeling and fired. There was a cloud of smoke and a great booming noise from the gun, such as I had not heard since I used to shoot black powder shotgun shells as a boy. I got a glimpse of a couple of the pigs as they ran away, trotting rapidly for a few steps, then hesitating and then going forward again. When the smoke rose we went to find the animal that had been shot at, but there was no sign of it. I could hardly believe that any one could miss a broadside at fifteen yards. There was not a drop of blood or a sign of a wounded animal. Later I learned that most of the black hunters that I met in the Cameroon were just about as good shots as this boy, notwithstanding that they all had charms which they carried in their belts or pockets to guide them aright.

Most of the forest here was thick. We had been walking from early morning. I was getting rather tired, and the boys had lost much of their enthusiasm for tracking. After our encounter with the swine we had been winding back and forth through the forest; it was cloudy much of the time and I had not kept track of our direction with the compass. It was impossible to tell where we were, and the natives did not seem to know.

It was about three or four o'clock when the natives suddenly stopped and turned partly around. I had heard nothing, but they claimed they had heard a gorilla bark. They said it was some distance away, but that unmistakably it was a gorilla, so we immediately set off in that direction. In perhaps a half hour we came upon very fresh signs of gorilla and a few minutes later I saw the bushes move. Here the forest was heavy, but still there was more underbrush than usual in heavy forests. The gorillas were in a group and quite close to cover. Suddenly a half-grown one walked right out into plain view and

then I got a glimpse of a very large one. Just as I raised my gun it stepped behind a tree. I could not see where it came out. Then there was another, apparently a large one, moving just behind some saplings and brush and there were still others. One was huge and once when it looked in my direction for a fraction of a second the coloring of its face and the hair of its head were reminiscent of a gigantic drill.

There were two or three big trees in such a position that the gorillas, the big male and the young one which had passed from my view, could come within a very few feet of me without being seen again, so I was alternately watching straight ahead where the group was and to the right to see when the big fellow would come from behind the tree. I saw one that had been moving a sapling fairly distinctly, but when it moved it was in the shade and hard to see. However I fired with a solid metal-patched bullet. There was an outcry and they all fled, the enormous male bringing up the rear. We quickly went in the direction I had fired, and I expected to see the animal stone dead. Instead there was blood.

We followed the trail of the feces, which are always abundant after any sudden disturbance. It was very exciting as we went along finding here and there a drop of blood and expecting to come upon a fallen gorilla. Sometimes the boys would hand me a leaf with a drop of blood the size of a pinhead and we would know we were on the trail of the wounded animal. Once this animal stopped and wiped the blood off on the leaves, and thereafter for some distance the trail was very difficult to follow. But with myself and the three boys scouting along, sometimes together, sometimes several yards apart, we were able to keep the trail very well, though we went very slowly. At about 5 o'clock in the afternoon we came upon a place where there were about a dozen gorilla beds. The group we were following passed right on over these.

The natives complained that we had better try to find our way out of the forest, as it was beginning to get dark and they did not know in which direction to go to find a trail. However, after passing these gorilla beds we had gone perhaps a half-mile when we came upon a well-defined path. It was impossible to follow the trail of the gorillas further. The natives went up and down the path for a little way and then seemed to agree that we should go to the right to find some village or clearing. I think it was not more than a mile to a village where I found my police boy and porters with my bed-roll.

After spending the night at this village, we started out at daybreak and followed the trail of the same band of gorillas. We came upon the place where they had slept. We could even locate the one that had been shot. My bullet had apparently missed the animal's brain by not more than an inch, for it had passed through the nose, and the gorilla's nose is very short. A mushroom or soft-nosed bullet would doubtless have torn the whole face of the animal and probably killed it, but would also have made it unsuitable for preservation in the way we wished. This day we found no more blood on the trail and the hole drilled by the bullet through the animal's face had apparently done little harm.

We came upon one or two other groups of gorillas, but apparently they had been hunted and were quite shy, so finally on the 10th of December I returned to Vimeli, having been out since the seventh. We returned to Yaoundé on December 14.

When we had first arrived at Yaoundé one of the missionaries told me that at a place far in the interior of the Cameroon, named Djaposten, there were many gorillas, that in one morning's walk of perhaps two hours he had

counted more than 100 gorilla beds. However, our stay in the Cameroon was longer than we had anticipated, and it was now time for Drs. Gregory and McGregor to start for home. Shortly after they left Yaoundé for the coast on their way home, I left on December 17 for Djaposten by motor-truck, the distance from Yaoundé to Djaposten being 541 kilometers (338 miles). We stopped that night at a native village called Sanken on the northern border of the great equatorial forest region of West Africa.

The road from Yaoundé runs along this northern border of the forest and has been continued eastward by the French and Belgians until now it joins with the roadways of Uganda and East Africa and forms a great continuous motor highway across the continent. This might lead one to think that there was much traffic over this road, but this is not the case. Several times in going back and forth between Yaoundé and Bertua, which is at kilometer 332 from Yaoundé, where the road turns south again into the forest, we met only two or three motors.

This is a very picturesque country. Even where there was savannah there were often gallery forests in the depressions. There were people scattered through this country, and at Bertua there were real savannah people with a somewhat different culture from those of the forests. They built large round mud-walled houses, with grass thatch. They kept many goats and sheep. The people themselves were darker and appeared to be more brachycephalic than those of the forest. This was especially noticeable when they were grouped together at the market. There were a few cattle here, but I believe they had been brought down from the north.

The country about Bertua and over much of the road between Yaoundé and Bertua seemed very fertile. In most places the soil was red and supported either a luxurious growth of grass and acacias or else rain forest. When it was possible to make a choice, the natives always chose to make their gardens where there had been rain forest, not in the savannah, perhaps because weeds would grow less at first in the forest. Beside the road in many places the natives, by order of the French Government, had planted plantains, bananas, cassava, caladium, sugar-cane, corn, sweet potatoes and other food plants, in order to supply food for people working on the road.

This was a fine road, though in some places the bridges were very poor. They were replacing the wooden bridges by ones of reinforced concrete. I often thought what an enormous amount of labor could be transferred from road-building to the production of food crops if the steam-roller were used to pound the road instead of the natives with wooden blocks; for it requires a great number of natives to carry gravel in loads on their heads and others to pound the surface of the road with wooden mallets, and nearly every foot of the thousands of miles of road in West Africa has been treated in this way.

The second day's journey from Yaoundé brought us well down into the forest. Early in the afternoon of December 18, 1929, we reached Abong Mbang and had just passed beyond the Government Poste when I was told that there was an American doctor, Dr. Lehman, at Abong Mbang. I ordered them to turn around that we might go to see him and then was told that his house was about 6 kilometers from Abong Mbang and that he was not at home but would be back in a month. So we continued on toward Djaposten.

Just at dusk we reached a rather large native village. The chief was a very tall Negro, very quiet, dignified, with very deeply sunken eyes, whose name was Atangana Impene. He was building a large new house, the frame-work

of which was already up. There were several rooms and a high-peaked roof and the latticed walls ready to be plastered with mud. This structure looked like an enormous and intricately-made basket, for there were no nails used in its construction. Everything was either crudely dowelled or fastened together with rattans. This chief had shoes and wore clothes like a white man. Later on he put a board floor in this house, the floor being raised up from the ground and a terrace built up around it on all sides. He later said that this was so that you could get that peculiarly pleasing sound that one gets when walking across the bare board floor with heavy shoes on in a white man's house.

At Atangana Impene's village the night was misty and quite cold. I slept in a new house, the mud walls of which were still damp, and there was a tendency for the mud of the floor to stick to everything. As in any typical native house, there were but two tiny windows close under the eaves. Even in the middle of the day one would hardly be able to see without artificial light. He had put this house up especially for European visitors. The rooms on either side had been closed, but the foyer had been occupied by goats and sheep, which made it smell like the rest of the village.

Since leaving Abong Mbang the country had become very hilly, almost mountainous. In most places the road was good, but in some places the steep hills were badly washed. We left Atangana Impene's village about 7 in the morning and reached Djaposten about 8:30. There were no villages between the two places. When the natives of Djaposten heard the sound of the motor truck they came from their houses, which were back a little from the roadway, and congregated along the road. The driver stopped where a group of natives had gathered. I got down from the truck and asked, "This is Djaposten?" A rather short, stocky, flat-nosed Negro with a large black felt hat, white trousers, khaki shirt and no shoes, stepped forward and offered me his hand. "Me be king for dem town." His name was Ngom. I asked him where the mission was, for I had understood that the American Presbyterian Mission formerly had one of its men stationed here. Several of the natives replied in unison, "Mission live for up." Therefore we climbed back on the truck, the chief coming too, and started up to the Mission a kilometer farther on. Most of the group followed along behind or beside the truck.

Here was another small group, one of whom was pointed out to me as the "man for Mission," this one, "black doktor," in other words, one of Dr. Lehman's medical boys, stationed here to dispense certain medicines to the natives. I informed him that I had been at the American Mission in Yaoundé and upon their advice had come to Djaposten to hunt gorillas. I told him I wanted a house. He said "White man's house is right over here." We were able to get the motor truck up to this house, where all my paraphernalia was unloaded. No white man had lived at Djaposten for some time.

At Lomie, thirty-six kilometers further on, there was an outpost of the French Government. I decided to go on immediately, while I had the truck, to Lomie and present my credentials to the French officials there. The chief of Djaposten, Ngom, asked if he could go with me to Lomie and Dr. Lehman's medical boy, Ntje, also wished to go in the truck to bring back a sick woman and child. So I told them to climb on the truck. Parts of the road between Lomie and Djaposten were especially steep and rough. I found the two French officials, M. Geandin, *chef de subdivision*, and M. Vidot, *agent special*. I showed them my credentials and told

them that I wanted to hunt gorillas in the neighborhood of Djaposten, that I wished to bring the gorillas out entire and for that would require a number of porters. I requested that the chief be instructed to supply me with twenty or thirty porters should they be required. This was done and I was also told that the government wage rate for porters was 2.25 fr. per day for 25 kilo load. They transcribed some of my papers, especially a letter from the Governor of the Cameroon. We started back to Djaposten early in the afternoon.

The following morning, December 20, with Ngom, one of my boys, Ndongo, and a local native, we hunted for gorillas to the north and east of the road. We started early in the morning and went over much swampy country and second growth. I saw six nests of gorillas, all of which were old, and one or two gorilla tracks that were fresh. We came upon a herd of *Potomochoerus,* and one of them, a large boar, I shot.

On the way back we saw an interesting fish-trap, where a stream was dammed with palm leaves and the fish, in trying to go down-stream, were caught on a lattice of palm petioles. Just before we reached this fish-trap, near the roots of a huge forest tree, there was a large rat that had been caught in a snare set in a hole under the roots. Ngom very deliberately crushed the animal's head with his knife and smiled as he slipped it in a fold of his cloth. I asked him if he had set the trap. He smiled again and said, "Another man made it." It had apparently been set by one of the men who had made the fish-trap. Ngom showed them the rat, and though I did not understand exactly what they said, it was evident that if a man wanted animals from his own traps it was his business to visit them early.

It was late in the afternoon when two natives arrived, carrying the boar on a pole, and everybody else in Djaposten arrived also, to help them take away the meat. I took what I needed for myself and what I thought would be sufficient for the boys, and then gave the rest to Ngom. For the rest of my stay at Djaposten, almost a year, I heard complaints about the careless way in which I had turned over the meat to Ngom, and that this man or woman had not received any. The following day I arranged with Ngom for ten carriers to take my outfit to some place up the Dja, where he claimed gorillas were abundant. About 9:30 a native came in with Ngom and said that gorillas had kept him awake all night at his clearing. We went with him about three miles to his clearing and hunted in the neighborhood for three hours without seeing any mammals. I collected a specimen of caecilian, a wormlike amphibian. As it crawled on the dark, damp leaves of the forest it looked like a giant earthworm, bluish-gray in color, about a foot in length. This is a rare animal in collections and Dr. Noble told me upon my return that this was the first specimen to be collected by a Museum expedition.

Much of the forest through which we hunted was large second growth, where the most abundant tree was the umbrella tree, *Musanga.* This grows to be quite a large light-barked tree with a palmately compound leaf and frequently with roots that branch from the trunk several feet from the ground, and look like braces to support the trunk. In this sort of country there seems to be an abundance of food for chimpanzees and gorillas. They both feed to some extent on the leaves, bark and buds of the *Musanga.*

I had expected to leave Djaposten on the twenty-second of December, but we again had to postpone going inland because a boy ate so much pig that he was very sick, and the others said they were Christians and could not work on Sundays. Finally we left Djaposten on the

twenty-third of December and followed a trail north of the Dja toward the east, and spent the night at a place called Malen, which consisted of one filthy hut occupied by two families. Malen was about two hours' walk from Djaposten, and the country was fairly level.

Large plantain-eaters are common in the forests here, called by natives "kunduk." Their calling in numbers is a common sound, especially in the morning and evening. They are beautifully colored birds, blue, green, yellow and black. A smaller variety is much less abundant. One time I shot one of these smaller ones for food. I had been told years ago that the red pigment containing copper in the wing feathers of these birds would soak out in water. I put a few of the feathers in water but after two or three days none of it had soaked out that I could see. The feathers lay around on a little stand that one of the boys had made for my washbasin. Some soapy water came in contact with the feathers, and the red pigment immediately washed out; putting it in soapy water I found that it tinged the water pink and the feathers also changed from red to pink.

We spent one night at Malen and then decided to move farther up to a place in the forest that Ngom called Kinoa-Mpanga. We left Malen early in the morning and traversed rather flat country and four streams. Near the streams were palm swamps. Most of the forest was heavy, perhaps some of it virgin forest. I saw gorilla and chimpanzee tracks in a swamp; in one place there were elephant tracks. Several monkeys were seen, most of them a species of *Lasiopyga* with a red tail and a white moustache, probably *L. cephus*.

Ngom and I led the way in the march to Kinoa-Mpanga. At one place where the forest was fairly thick, Ngom turned to me and said "Antelope," and pointed. I knew from what he said that the animal was quite close. I froze and hesitated for several seconds, trying to see it but I could not until it leaped away, when it was too late to shoot. It was a red forest duiker.

It was just after mid-day when we arrived at Kinoa-Mpanga, a place in large second growth but quite close to original forest. The closest permanent dwelling-place was the hut we had left that morning. At Kinoa-Mpanga were five huts built, about a year before, by Ngom and his people when they had come to hunt and gather rubber. The huts were only a few yards from a stream which is a northern branch of the Dja. That afternoon Ngom and I hunted northwest from camp until dusk. We saw a number of monkeys, squirrels and large horn-bills but no gorillas nor chimpanzees, though we did see some beds.

On Christmas Day I was hunting with a powerfully built young fellow, named Nkoul, and Ngom. About noon we came to a palm swamp. These swamps have a stream meandering through their center, and from the stream to the edges of the swamp there is mud from a few inches to a foot and a half deep. The palms, beautiful and enormous, were raphia palms which do not form trunks. I usually took off my boots and socks when I came to one of these swamps in order to have them dry when I reached the other side. I had just taken them off on this occasion, when a squirrel, saying "ch-ch-ch-ch-ch," ran up on a palm leaf and then jumped to another on a nearby tree, the hairs on its tail all standing erect. I tried to get a shot at this animal with a .22 rifle, and in order to do that I followed it toward the center of the swamp. By the time I had reached the stream the squirrel had disappeared, Ngom was beside me, but Nkoul had disappeared. I inquired where he was and Ngom called. When Nkoul answered, Ngom turned around and said "Small beef," which may mean

any sort of small animal from a mosquito to a goat. I said "What small beef?" and he answered "Nyo," which was so much like the Kiswahili word *nyoka* for snake that I recognized it and followed him in the direction of Nkoul's voice. As we approached through the soft mud on which were scattered palm leaves I saw the body of a snake that was about seven inches wide. At the first glance I thought it was a python and said to Ngom, "Where is its head?" Then I recognized by its scales and color that it was a Gaboon viper. Ngom meantime had walked right by the body of the snake within two feet, and in answer to my question pointed straight down, saying "Here is its head." Nkoul had cut a sapling about an inch and a half in diameter and five feet long. Now they split one end of it and sprang it open, then kicked the leaves off the snake in a most careless way so that I warned them, "This snake be very bad," to which Ngom answered, "Ya. Snake he bite, man he die." They then proceeded to place the open split end of the sapling over the snake's neck just behind the head and gave it a sudden sharp thrust down into the mud. With that the snake writhed and opened its mouth, and as they raised it up with its neck wedged in the stick its fangs were erected and it squirted venom several feet.

They told me that this snake was very fine eating, and indeed the flesh was very delicate, tasting more like frogs' legs than anything else. When we reached camp I preserved the skin and head. The animal measured just five feet in length. Its flesh was white and very delicate. For dinner that evening we ate the flesh of the viper and some prawns. Both were very good. The prawns had been caught by some of the women who had followed in the wake of their men folk. The thought of eating a viper seems repulsive to people at home and it would in camp, too, if there was any one about to make fun of it or turn up his nose at it; but when meat is scarce and every one is saying what fine meat it is, then one goes by the taste.

Two days later another squirrel chattered and another viper was discovered after a good deal of search, lying in the leaves not fifteen inches away from the spot where I was standing. This was likewise secured with a split sapling and was duly eaten in its turn. Ngom claimed that in the daytime several men walking Indian file could each step on that snake and the snake would not move, but that at night it was a very different story, because the vipers are very active then. Indeed the natives were so much afraid of the vipers at night that most of them refused to go out with me at night in the forest, as I frequently did whenever gorillas were known to be nearby.

I had taken along many more carriers than were necessary for this camp, for I expected to get a gorilla any day and would require them to carry it to camp and back to Djaposten. However, day after day we hunted without success. There had been a dozen or fifteen men in camp. Ngom had ten or twelve wives, two of which followed along when we left Djaposten to carry his food and belongings. But every day or so one or two more would arrive bringing food, while some that had come out first would disappear. A number of the porters had their wives come out with food, some of the women also bringing children, until our camp numbered about thirty people. It was interesting to see them at close quarters.

I was amused when Ngom and I returned to camp in the afternoon or evening at the casual yet dignified way he would greet any of his wives that had arrived during his absence. They would come up and offer their hands, which he would take with some word of greeting. The women would then turn and walk away. Later on the new arrivals would invariably bring him food that they had

prepared. One of them who had come out with us from Djaposten had not gone back but stayed in camp. This one was an older woman. I understood that she was Ngom's oldest wife and she acted as though she were the head of the household. Sometimes when a new wife arrived this woman, who could speak pidgin English, told me that this was another of Ngom's wives.

Frequently the women would catch small fishes and prawns in the stream by selecting a place where there was a bend and the water was shallow and where there was an overhanging bank. Here they would make use of all sorts of debris, sticks, leaves, logs and mud, to build a dyke, thus walling off a little part of the stream. When they had this enclosed they stood in this with their feet wide apart and using a flat basket as a scoop, bailed out the water. Then they wallowed about in the mud to find the many varieties of fishes and prawns. Characins and cat-fish predominated. I made a habit of selecting specimens of the various types they collected, but frequently by the time I reached camp in the evening they would already have smoked the day's catch upon the little woven mat-like structure which they hung over the fire.

On several occasions the men tried their hands at fishing, but they were not satisfied to do it as the women did. The men made basket-like traps which they placed in the stream, usually two or three abreast. The mouth of this basket-like structure headed up stream. Then men stood beside this basket with long knives, while other men were armed with very long saplings, the large end of which had been pounded against rocks until the whole end was mushroomed and brush-like. Starting far above the point where the baskets were placed, the men stabbed at the water with these poles, each thrust causing a loud *"pwok!"*, which must have made a great concussion in the water, for fishes hiding under the bank would dart out and flee downstream away from the disturbance. They would be driven into the traps or often killed by the knives of the natives carrying the traps as they tried to dart by. The commonest fish caught in this manner was a long-snouted characin about a foot in length, which was fairly good eating.

The people at Djaposten were very poor. At the time of my visit they had no means of disposing of anything for money to the traders at Abong Mbang or Lomie. Previously they had been in the habit of gathering wild rubber and wild coffee. I saw a number of wild coffee trees in the forest. But the traders were not accepting either the coffee or the rubber at the time of my visit. Occasionally I could get them to bring a few eggs from Djaposten. However, the commonest food that they would bring out would be large plantains, cassava, caladium and, once or twice, sweet potatoes. Consequently meat was scarce. I always took a 22-caliber rifle as well as a high-powered rifle with me when hunting. Then if there were no gorillas about I would shoot monkeys, which we ate. The flesh of these animals was very good, much better in fact than any of the goat's we had had in East Africa, though the young goats and sheep that I had in the Cameroon were very good.

One or two of the natives hunted with crossbows during the day. Once one killed a monkey, but as a rule they came in empty-handed. I was getting dissatisfied with this locality, and one day Ngom asked permission to go to another place some distance off on the south bank of the Dja. He was gone from dawn to 2:00 P. M. When he returned he had with him two fine monkeys he had killed with his crossbow. He told me that he had seen gorillas and that we could surely find them there.

On December 30 we started soon after daybreak and followed the path south toward Malen for about two miles, then we turned southeast and crossed the Sa

and later the Dja. We took embalming fluid and several natives as far as the Dja. Then Ngom and three of his best men went on with me, crossing the Dja on the trunk of a fallen tree. Ngom and one of the others carried their crossbows.

Beyond the Dja was a palm swamp where we saw gorilla tracks. Beyond that we hunted through heavy forest and second growth alternately. At 11 A. M. we came again to the border of a palm swamp, where there were fresh gorilla tracks. There was a chimpanzee nest, that I photographed, in a tree just at the edge of the swamp.

As I was doing this we heard a gorilla in the distance, and a few minutes later started in that direction. Ngom and I went along, leaving the other three behind. It was difficult to follow this gorilla through the heavy forest; for yards at a time we might be led onward only by a faint knuckle mark, a broken twig or a bit of chewed-up *Aframomum* plant. Many times we were quite far from the trail when we heard a branch break or some vocal sound that led us back toward them. They moved about in the forest as quietly as a cat walking across a carpeted floor. Frequently we got close enough to them to see the vegetation moving, sometimes in two or three places at once. Then apparently the group would move off, while one would remain behind. We would think that the whole group was still before us. Then apparently the one behind would go off, without our knowledge, of course, and we might wait for some time believing them close by. Then we would walk very carefully forward, taking advantage of all cover, only to find that they had left perhaps 10 or 15 minutes before. One time we were quite close to one when nearly a hundred yards ahead another had climbed a large tree bearing fruit about the size of a croquet ball. We could hear the swish of the leaves and then the beating of the fruit as it struck the ground like falling apples. However by the time we were able to get close to the tree, they had gone on.

About 2 P. M. we had been close to them for some time. I saw vegetation moving, and at one side there was an open place which I felt sure they would come into. I had my rifle raised, ready to fire, when a large antelope walked out. Apparently it had been hiding there and had been disturbed by the gorillas just beyond. Finally late in the afternoon we succeeded in getting ahead of this group in the direction they were going. Then for no apparent reason they went off at right angles. We followed; we saw their arms move on one or two occasions and I followed, absolutely sure we would get one or two of these gorillas. We had been very close to them for a long time and it seemed impossible that they could avoid coming into view. They seemed to be following down a little depression.

We made a detour, following back trails and were just at the edge of the depression when we heard gorillas in several directions. Suddenly I turned to the right and there I saw facing me a gorilla, standing in the open, but it was not an adult. It was pure black and about the size of an adult chimpanzee. We were face to face. I aimed at its head and then I realized that it was not the gorilla I wanted. It looked at me a moment, then turned and went on down the depression, not going back with the rest of the group. As soon as it was out of sight it made a noise like beating its chest or clapping its hands, and instantly all the others were silent for a minute or more. This was followed by a terrific roar and stamping by a male, and we waited, expecting them to charge. We did not wait long, however, for I knew they might go away and this was what they did. We followed a short distance to find that they had been frightened, as shown by the feces. It was then too late to follow further, for the sun was low. By walking very fast we reached camp just after dark.

On December 31, 1929, I loafed in camp until late in the afternoon. Then I went out to hunt for meat for dinner. I shot a hornbill and a red-tailed monkey, a female with active mammary glands. I asked Ngom who had made these clearings round about our camp years ago, where there is now such large second-growth forest. He told me that during the war the Germans were occupied in fighting and could not control the natives. Consequently many natives had run away and it was some of his own people that had come away off here, far from any village, to establish themselves away from the white man's demand for labor.

One morning the natives asked me if I had heard gorillas talking during the night. I said no, that I had never heard gorillas talking. They said that gorillas had talked at night and that they had heard them. I told them that in the future if they heard gorillas talking they were to wake me, and a night or two later they did so. There was a sound like the twang of the string of a bass viol, very deep and resonant. I presume that this noise was made during exhalation, accompanied by a rapid vibration of the lips. I kept track of how often this sound was made and took a compass bearing on the direction it seemed to come from. The following morning we set out in that direction and eventually found a place where gorillas had slept, but were unable to trail them more than a short distance through the comparatively dry forest where their feet made no impression.

The weather continued clear and dry in the middle of the day, but by midnight it was cold and the forest dripping like rain. This continued until after sunrise. In the heavy forest the leaves of the forest floor do not get crisp before about noon. Mist or low clouds often obscure the sun until 10 or 11 A. M.

While hunting gorillas I took the .22 rifle along and occasionally shot a monkey or a hornbill for food. On two or three occasions I got forest guinea-fowl. Several times we saw tracks of the giant forest hog, *Hylochoerus*, and more often those of the red river hog, *Potamochoerus*.

Nearly every time that I saw elephant tracks I could also see the tracks of pygmies who had been hunting the elephants. Whenever these little people are lucky enough to kill an elephant or wound it severely, they will follow it until it dies, and then call their companions, who come and live where the elephant drops until it is eaten up, as it is easier to move a pygmy village than an elephant. They then carry away the tusks, which they take to the chief of some village and trade for iron and perhaps gun powder or cartridges. The French government allowed certain of the native chiefs to have shotguns. These were not ordinary shotguns but old bolt-action army rifles bored out and made over into twelve-gauge single-shot shotguns. The chiefs in turn sometimes turned over such a gun to a group of pygmies, who were ingenious little hunters. They would procure meat and ivory for their larger Bantu neighbors in exchange for peanuts, maize, bananas, and so forth.

On only one occasion did I see the tracks of buffalo.

Sometimes the natives would get honey from bees' nests within tree trunks. One afternoon I got one young of a large species of owl as it flew from a tree we cut down for the honey it might contain.

It was with difficulty on one or two occasions that I tried to go in the direction of the gorillas at night. The natives disliked walking in the forest at night, and some absolutely refused to do so. I was never successful in getting near the gorillas at night. Either they would stop talking or we would find that they were too far away.

On one occasion I left camp alone very early in the morning, expecting to return within an hour or less, when I

came upon fresh gorilla tracks and followed them. Sometimes I could follow their footprints; at other times I heard their voices in the distance, so that I was guided alternately by their voices and footprints. At times I was so close that I could hear the vegetation rustle, so I continued on their trail throughout the day. Late in the afternoon they went up a large tree for fruit and only just came down before I got there. This was in the densest sort of jungle; many of the larger trees had been blown down by a storm so that there was just a tangle of fallen trees overgrown by lianas and rattans. It was clear that I could not get back to camp, as beneath the dense canopy of vegetation it was already getting dark. I therefore looked for a place to sleep and found a tree which probably had been struck by lightning and splintered. I could break off bits of the wood and make a fire. I gathered large leaves which I flattened beside the fire and spent the night right there. I thought if I fired a shot there was a bare possibility that the natives in camp might be able to hear it. Fortunately a wood pigeon came nearby and the shot served two purposes. The pigeon, struck with a heavy rifle bullet, was almost blown to bits, but what remained I made a meal of.

I had begun to doze about nine o'clock when I was suddenly aroused by a noise which I recognized as a gorilla talking, but this noise was as loud as the roar of a lion. It seemed to me that these gorillas could not have made their nests more than a hundred yards away from where I lay, so loud was the noise. They may repeat this talk or call or tremendously amplified growl many times. On this occasion of course I felt here was my great chance. If I had had an electric torch or other light I could certainly have found the place where those gorillas were sleeping, but as I was situated I could do nothing. During the night I got so cold that I divided the fire in two and slept between the two fires, as dew continually dropped from the trees above. At dawn I started hunting for the gorillas, hoping to catch up with them before they had gone far. However, in the dense underbrush it was two hours before I located the place where they had slept. I had had only a handful of peanuts and the pigeon to eat, therefore gave up the pursuit of the gorillas and tried to find my way back to camp, where I eventually arrived about noon to discover that most of the natives were out looking for me, but none in the direction from which I came. They claimed they had heard the shot that I fired the evening before, but apparently no two agreed on the direction of the sound.

(The next section entitled "Men, Gorillas and Sleeping Sickness" by H. C. Raven will be printed next month and will conclude the series "In Quest of Gorillas.")

FOSSILS IN PENNSYLVANIA[1]

By Dr. BRADFORD WILLARD
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IT has been my experience as a geologist that the average American citizen's knowledge of fossils varies greatly in different parts of the country. Some might attribute this to climate, innate intelligence, ancestry or occupation. I offer no explanation, but merely record my observations drawn from experiences ranging from New England to California that some parts of the population are more conscious of the presence of fossils than others.

Mention fossils to a western rancher, and the chances are that he will produce from somewhere about his shack a few fossil bones or teeth that he has picked up thereabouts. Finding this common bond of interest, he may volunteer to lead you to some neighboring "draw" and point out fragments of an ancient skeleton protruding from the partly eroded bank. Take one of these chaps in the field with you on a fossil hunt, and you may be surprised by his familiarity with details of these long-dead creatures. But mention fossils to an easterner, and observe the mental reaction.

Let us suppose that you have just answered negatively the standard question, "Hey, mister, lookin' for gold?" and told your inquisitor that you are digging fossils. If he still evinces any spark of interest, he may inform you that he has some arrowheads at home or that his cousin found some "mineral rock" in a cave "back in the hills"; or he may even confide in you that he knows somebody who has a collection of old postage stamps! Needless to say, these articles are hardly to be classified as fossils. Not long ago a certain brand of gasoline was advertised by "stickers" depicting dinosaurs. While buying this brand of "gas" at a station in central Pennsylvania, I casually told the attendant that I had found, within a few miles of that very spot, the foot tracks of just such animals as the "stickers" showed, imprinted in stone. Aware that dinosaurs were reputedly large, he allowed that they might have been heavy enough to impress their tracks in the solid rock. Sceptically polite, his feeling of relief when I drove off without becoming violent must have been profound. Nevertheless, be it due to the vicious effect of heredity or not, my small son collected the "stickers" with avidity. Then, while accompanying me on his first visit to the American Museum of Natural History, he astonished to muteness an obligingly attentive guide in the Hall of Reptiles by pointing out with no uncertain gesture and in unsubdued tones such examples as *Triceratops, Stegosaurus* and *Trachodon*. The gasoline "stamps" had done one bit of missionary work.

But, just as all westerners are not necessarily born paleontologists, so all easterners are not indifferent to fossils. Sometimes in the field I have met one "just queer enough" to care about rocks even if they never put a penny in his pocket. More than once it has been my pleasure and good fortune to go a-field with such, and I assure you that the benefit drawn from their enthusiasm over the local geology far eclipsed what little "scientific" return I was able to make. Now and then into my office comes some one wanting to know about the rock full of "petrifications" he has brought to show me. The chances are

[1] Published with the permission of the State Geologist of Pennsylvania.

that my visitor is either a farmer or a Boy or Girl Scout; for, after all, these seem to be almost the only non-scientifically trained people nowadays who have any particular interest in what their eyes show them outside of the cinema houses. And so, I set aside my work and try as best I may to explain that the shells or leaves in the rock are actually the remains of prehistoric animals or plants which once lived in Pennsylvania. And I tell my visitor how these fragmentary remnants entombed in the rock once lived and that then the rock was loose sand or soft mud which later hardened after the dead remains were buried. Call it proselytism if you will, but perhaps there are just enough people interested so that the account which follows may not prove entirely a journey through "the valley of dry bones."

The majority of the rocks of Pennsylvania (unknown to most of us) are probably fossiliferous, that is, they carry enclosed within themselves the remains of strange, long-dead creatures and plants which once constituted the leading citizenry of the Commonwealth. Tell this to most people, and they'll cut you off their calling lists, where only "rational human beings" belong. Yet, wherever one encounters rocks formed in regular layers, even though these layers or beds are tipped and broken, bent and twisted, if they be not too badly damaged, the chances are that you may find fossils in them. At least it is worth while looking over the flat surfaces, and perhaps supplementing this labor by hammering to pieces a bit of the rock. Usually, it will break into layers, and perhaps on some will be found the delicate tracery of a "fern" leaf, the beautifully symmetrical, many-ribbed imprint of a sea-shell or a curious foot track. Our fossils are not confined, you see, to the plants so commonly brought back as "souvenirs" by visitors to the coal fields. On the contrary, these coal plants, interesting as they are, constitute but a small part of the fossils discovered or discoverable in Pennsylvania. The rocks within our borders contain remains of nearly every common kind of marine animal, besides fossils of land-living, air-breathing creatures of many and diverse sorts. Fossils can be found in almost all counties of the state except the southeastern corner. Here the rocks have been so crushed, broken and changed that all semblance of fossils, if any ever were present, is probably destroyed.

How came these fossils to be in the rocks of Pennsylvania? Long, long ago, the geography of what we call the Keystone State was far different from that of the present. You had already guessed this when I told you that our bedded rocks contain fossils of animals which lived in the sea. What was that sea? No, not the Atlantic Ocean. This geography of millions of years ago may be pictured if we imagine the Gulf of Mexico, Hudson Bay and the Gulf of St. Lawrence to be extended mutually toward each other until they meet somewhere in the Central States. Then let these salt waters spread widely, and they will produce a vast, shallow, inland sea larger than anything of its kind now on earth, but representing a condition not uncommon ages ago. The eastern shore crossed Pennsylvania in a southwesterly direction; and, eastward therefrom, none knows how far, stretched a land which geologists have christened Appalachia. It must have occupied much of that region of the globe over which to-day spread the waters of the western Atlantic. Every one knows that the rivers of eastern Pennsylvania flow eastward or southeastward toward the ocean. The rivers of Appalachia ran westward into the inland sea. Their direction was essentially the opposite of our Delaware, Schuylkill and Susquehanna. Laden with waste washed from the land, they

carried their burdens into the ancient, Mediterranean-like body of water and there laid them down as sheets of mud, gravel and sand spread far over the old sea floor.

Our solid rocks to-day are the hardened remnants of those very sheets. In them we trace here and there the ancient shore-line with its sandy beaches and spits, its gravel banks, rippled sand-bars and muddy estuaries. Or, we may discover an ancient delta where some now extinct river left its load of ground-up rock brought down from an inland mountain chain. Farther out from the old shore-line, we find the remnant of ancient coral reefs where it bordered sheltered lagoons. Suppose, in imagination, we exchange our hammer for a deep-sea dredge such as is used for collecting samples of mud from the ocean bottom. To complete the illusion, let the family "flivver" in which we have gone fossil-hunting become a sea-going tug. Then may our rock specimens be likened to mud samples from the ancient sea bottom. Farther and farther from shore we cruise. Now our dredge brings up black mud, the antecedent of our slates, and again it drips limy ooze which we recognize to-day hardened into one of our many limestone formations.

But, did all our rocks originate on the sea bottom only? No, not all of them. Those vast deposits of coal and associated shales and sandstones filled with plant impressions which make up the dominant rocks of our coal fields are of no deep-sea origin. These are the hardened gravels, muds, sands and matted accumulations of dead plants which formed in river beds or on their flood plains, in piedmont regions and along low, swampy, coastal flats. No one need be told that the salt sea which once covered so much of the interior of our continent is no longer there. Part of the explanation for its disappearance is to be found in the fact that the sea was finally filled, at least along the borders, with the sediments the rivers brought in or the waste which storm waves tore loose from the coast and currents dragged out to sea. Where the sea was filled, coal-bearing rocks and other formations of non-oceanic or fresh-water origin (called "continental deposits" by geologists) came into being.

"But," you comment, "this may be instructive to some, but so far I have waded through several pages of an uninteresting recitation of dull statements about ancient and literally long-buried topics. I am as incredulous as was your 'gas' station attendant, and echo the Mock Turtle with, 'What is the use of repeating all that stuff, if you don't explain it as you go on? It's by far the most confusing thing *I* ever heard!' "

May I, without offense, presume to paraphrase so far as to say, "By their fossils ye shall know them"? There is the key explanation of most of our deductions. First, of course, we do examine the character of the rocks themselves. That tells us much. Dissolve away the material which holds together the pebbles in this piece of conglomerate or "pudding stone." What remains? Gravel, to be sure. Do the same with this sandstone; the answer is, sand. Could we likewise soften a specimen of shale, the result would be mud. Who has not seen gravel, sand or mud accumulating in half a dozen different places and under as many unlike conditions? Can you not from this hazard a guess as to how these rocks came to be? And now suppose that they contain fossils. We have a much better means of discovering their origin. Our ancient sea was populated by myriads of "shell fish" whose forms and habits are comparable to the forms and habits of their living descendants. As in the seas to-day, so in the waters of long ago, some animals lived in the shallows, some in the deeps, some near to, others remote from the coast.

Different forms inhabited the beach, the tidal marsh or the lagoon. Many animals spent their lives anchored in one spot, and there were those which crawled on the ocean bottom. More passed their days burrowing in the ooze and mud. Still other forms swam gracefully about or drifted helplessly, carried by wind or current. By analogy with to-day's marine population, we deduce the habits and hence the surroundings of the animals whose fossil remains we see in the rocks of Pennsylvania.

Likewise, or conversely, our fossil land plants show not merely that the associated coal beds formed on land, but give a clue to the ancient climate. Plants have been called "the thermometers of the past." Unlike animals, they can not move about and escape unfavorable climates; they must learn to live under new conditions or perish. Consequently, the characteristic plants found fossil imply something as to the temperature and moisture of the ancient climate amidst which they flourished.

So we obtain from our fossils an inkling of the conditions under which the rocks that imprison them were formed. Be the enclosed remains those of deep-sea dwellers, the rock originated far from land, but if the fossils are only land plants, quite the reverse is true. But, suppose a rock contains a mixture of leaves and sea shells? We may assume its origin to have been in the sea but near shore where a river brought plant débris down to become water-logged, sink to the bottom and be buried with the remains of sea life.

Such relations as these may be studied in Pennsylvania's rocks. Generally speaking, if one starts in the southeastern corner of the state and goes north or northwest into the nearest coal fields, he passes, with few exceptions, over a series of rock formations which lie successively one above another, that is, the farther we go, the younger are the rocks which we cross. The difference in ages of the extremes of this rock series is reckoned in hundreds of millions of years. Therefore, we are not surprised to find that the fossils in the rocks change as we go from formation to formation. Each is characterized by its own, individual sorts of fossils or groups of fossils common to no other formation. Organic evolution was constantly changing the character of the population, and each group of rocks carries a different cross-section of the evolutionary stage fashionable at the time it was laid down. The fossils are convenient earmarks by which we separate and recognize the several formations wherever we meet them. As we pass from older to younger beds, a profound life-change is seen. Our oldest fossiliferous rocks have, as their highest type of animals, certain "submarine vermin," small crustaceans no more to be reckoned with than so many good-sized shrimp. But, as we rise in the rock scale, and simultaneously scramble up the ancestral tree, we may be surprised to discover the bony armor which once protected the body of an ancient fish-like form. With this discovery we have reached the rocks which formed at the beginning of the age of backboned animals. If we go still higher in the scale of rocks and life, we (at least in Pennsylvania) turn to our coal deposits and those other related rocks which formed in later days, not in the sea, but on land. In them we encounter "bigger and better" fossils. The plant-bearing shales of the coal measures are occasionally dimpled with little foot tracks; foot tracks of crawling, creeping, salamander- or lizard-like folk. Here are forms above the fishes, but still "far down in the animal scale" as we complacently say in self-adulation. The many leaves and stems found with them, and the ancient tree roots embedded in soil now changed to solid rock show wnere the original "Penn's Woods"

raised its green crown toward the sun. Now and then, though very rarely, for they are extraordinarily delicate fossils, we may find among our coal plants a "leaf" more curiously veined than all the rest. That "leaf" is actually a wing! Not the feathered wing of a bird, for birds were not "invented" then, but the wing of a large, primitive insect which bumbled its clumsy, leaf-eating course among the old, flowerless plants or delighted to tickle the noses of the "creeping things" in swamps where coal was a-making.

The coal-time plants and animals are very ancient. And yet, though they lived some hundreds of millions of years ago, theirs is the last well-recorded chapter of earth history to be read within our borders. Father Time writes his record on stony pages; the characters which flow from his never-failing pen are the fossils. But, many pages, whole chapters or volumes, may be missing. So, if we would read all the rest of the story and understand what happened since the coal was formed down to to-day, we must roam far abroad from Pennsylvania to where a more complete record is preserved. But we have been permitted to take at least two glimpses of some of the later pages of Time's book, pages which that devastator, Erosion, has not ripped out and tossed away from the state's rock library.

The broad band of red sandstones and shales which runs from Gettysburg northeastward to the Delaware Valley preserves, though in a sadly fragmentary state, part of a chapter taken from that volume of earth history on whose title page is written "The Age of Reptiles." In these rocks one now and then finds a curious three-toed foot mark some three or four inches long. Occasionally, a row of these shows where an ancient Pennsylvanian picked his way across the mud flat of a now long forgotten river. Would that it were possible to take up the trail, track down and capture at least his fossilized skeleton! But, in Pennsylvania, a few bits of broken bones and stray teeth are all that these red rocks have yielded of the track-makers. They are, nevertheless, recognized as remnants of that surprisingly dull-witted and usually, perhaps appropriately, greatly oversized group of reptiles, the dinosaurs. It is an age-long gap from their time back to the period of coal-forming, but it is a far greater interval from the days of the dinosaur to the last chapter in Pennsylvania's record of earth history.

Thousands of years ago, a very short time compared to the hundreds of millions of years during which the earth has been circling about the sun, North America was in the grip of the Great Ice Age. Then a greater part of Europe and North America became temporarily ice-buried, just as is Greenland to-day. But, despite the chilling, "normal times" eventually returned, and events jogged on as usual. Now, this great ice-forming epoch happened such a short time ago that much of its record is still intact, unspoiled by time, so that we fancy we know a vast deal about it. Over much of the northern half of the State of Pennsylvania, there ground its way southward a giant ice sheet. Slowly it advanced from its far northern base of supply. Then for a time it melted back again, perhaps into Canada, advanced at least once more, melted again and finally disappeared altogether. Naturally, with all this uncertainty of purpose on the part of the ice, the equability of the climate suffered terribly. When the glacier advanced, the weather was so abominably cold that the musk ox trotted down into the Southern States and the walrus lolled on our Atlantic beaches. But, sometimes when the ice drew back, the climate improved remarkably, growing milder than that of

to-day. Then, such southern forms as the peccary and jaguar ranged north into regions to which, outside of "zoos" and circuses, they are to-day strangers. Here, again, fossils enter the story.

Because the ice never came quite as far south as the latitude of Reading, we have beyond its southern border an unglaciated country where contemporaneous animals and plants then living in Pennsylvania could leave their fossil remains. Can you imagine elephant-like creatures munching the foliage in Chester County? It is true that the mastodon and other even stranger beasts roamed unrestricted where now our domestic animals graze. Over half of the forms found fossil from the glacial time are probably now extinct; many others are to-day represented only in far-off lands by surviving descendants. They are gone from their former haunts in the Juniata Valley. No longer do they make winter quarters about Valley Forge as once they did, long before the advent of the Continental Army. In caves or crevices in the rocks have been found remains of mastodons, giant sloths, saber-toothed tigers, huge bears, a host of smaller beasts, a few birds and many kinds of leaves. Perhaps these write the last paragraph in the closing chapter of earth history recorded by fossils in Pennsylvania. Yet there is a possibility that we may add still another sentence; or only just a footnote. Perhaps you are asking, "What about man?"

Our answer is, "Wait and see." We are not sure. There are doubtless tons of arrowheads, corn grinders, net sinkers, clay pots and tobacco pipes of aboriginal manufacture accumulating dust in our museums and attics or proudly displayed on center tables or in "whatnots." But they are all brand new objects and no more traces of fossil man than King Tut's mummy, Westminster Abbey or William Penn's house. They are not prehistoric, but were made by the same race of "red" men whom our ancestors found here making appropriate to the American wilderness the adjective howling. Fossil man has long been known in Europe. Recently, evidence of his presence in parts of North America has begun to amass. In the West, certain flint "points" of peculiar design unlike any known to have been made by the Indians have been found mingled with bones of extinct mammals. But, disregarding our own venerated though largely discredited Lenapé Stone, we have no very sure evidence of the occurrence of pre-Columbian man in Pennsylvania. Some day, perhaps, a farmer will walk into my office or some other luckier fellow's office, with one or a bushel of these curious flints, and when he does, we can add at least this item to our history of fossils in the state. Even lacking such a final bit of proof and allowing for all the gaps in our rock record, is there not enough in what we do have to furnish "a source of innocent merriment"? Have we not here the means of profitable amusement, if not real research, for those who will inquire about the fossils of the state in which they live? Here, indeed, should be entertainment for those who care to ask themselves and others, questions about our rocks, their contents and their history.

THE STOMACH AS AN ORGAN OF SOCIAL ADJUSTMENT

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In that new biography of Joseph Conrad, written by his incredibly patient wife, there is the record of a visit by H. G. Wells and Bernard Shaw. Wells, Mrs. Conrad remembers, had a headache and would eat nothing but a slice of dry bread, washed down by a glass of quinine water, while Mr. Shaw made his meal off a cup of cocoa and a dry biscuit. Genius doubtless, like an army, travels on its stomach, but what a stomach!

The reason that we stand in awe of the stomach is that this organ, more than any other of the viscera, controls our social adjustment. Many years ago the wisdom which directs our medical school ordained that lectures in anatomy shall be given either at 8:30 in the morning or at 1:30 in the afternoon. At either of these times it is evident to the discerning and sympathetic teacher that all is not well with his charges. Their spirit doubtless is willing, but their flesh is very weak. Hence, some twelve years ago, Miss Kuenzel and I set ourselves to investigate the natural history of the stomach, hoping eventually to draw up some schedule of its habits in work and play, its moods and eccentricities. We did not care particularly about the stomach of animals nor even about the stomach of the sick. We were content to leave the elaboration of first principles to the physiologists and the discernment of disease to the clinicians, though we had some curiosity as to why the dim grey light of a February dawn, made famous in history by the execution of Mary Queen of Scots, should have so intensely depressing an influence on the human race and why the early Christian Fathers originated Lent. Our attention in the main was fixed on just what goes on in the secret recesses behind the rows of vests confronting us with more or less bombast every early morning and every early afternoon through eight months in the year which end with examinations scheduled, by some pedagogical demon now forgotten, in the very time of year when, from the nature of the season, young love might reasonably be expected to have least stomach for them.

In the course of time we did indeed draw up a modest tract which, if it did not satisfy the appetite of grim unhumorous scientists, did at least tickle their risibility. We gave it the title "Behavior Patterns of the Alimentary Tract." But even in 1930, when that volume appeared, the idea of behavior patterns in the stomach seemed preposterous to almost every scientific mind. Slowly, however, it has come to be realized that the stomach does have registrable moods and method in its manner of working, a knowledge of which contributes not a little to the brightness of life.

The stomach is a muscular organ, the activity of which is expressed in rhythmic waves of contraction which pass along it analogous to those waves of contraction which pass over the heart and are known as the heart beats. Waves of gastric contraction are called peristalsis and their rate is much slower than the rate of the heart beat. But inasmuch as the heart beats force the contained blood onwards into the blood vessels it has been assumed that peristalsis of the stomach occurs for the purpose of forcing onward the gastric contents. Furthermore, on this hypothesis, it has also been assumed that the activity of the stomach would be greatest during and after a meal and that between meals the stomach would be at rest. Another simple notion about the

stomach is that it secretes an acid digestive fluid for the sole purpose of starting digestion in the food, which later is propelled into the intestine by the peristaltic waves.

No one had actually seen the stomach at work until the invention of x-rays, which permit us to look through the body and watch the activity of the internal organs. It is true that the stomach had been watched during abdominal operations, but something interferes with its activity in these highly abnormal conditions so that movement is reduced to a minimum. It is also true that gastric activity had been observed in experiments on animals, but again this involved opening the abdomen and disturbing the conditions under which the stomach normally operates.

By mixing with the food a substance opaque to the x-rays and without influence on gastric activity, barium sulphate being usually used for this purpose, one can now easily study the movements of the stomach and, what is more surprising, watch also the accumulation of gastric juice within the organ.

This procedure is regularly carried on in the hospital to enable the clinician to observe the shape, size and movement of the stomach, but inasmuch as healthy people do not usually go to hospital the study there is almost always made on diseased or functionally disordered stomachs. It was not till Miss Kuenzel and I, with the active assistance of our medical students, undertook the routine systematic study of gastric activity, that this investigation of healthy stomachs was carried out on a scale large enough to justify conclusions on what actually takes place in the stomach free from disturbances of form or function. More than 800 medical students have assisted us in this work, so that the detailed study of their stomachs, added to the two which Miss Kuenzel and I possess in our own right, a study which has been prolonged for hours at a time and continued day after day at all seasons of the year, enables us to speak with confidence concerning this shy and somewhat irresolute organ.

When we first began to examine the student stomach eleven years ago we were impressed by two features, namely, its large size and its inactivity. Of course we did not realize that we were dealing with stomachs unused to being spied upon in this manner. We started the work at the beginning of the session in September, 1925. By February of the next year we began to realize that these stomachs which, in September, were so large and sulky were now smaller, even though we gave exactly the same amount of food as before, and more active than we had found them earlier in the session. By the following September these same stomachs had greatly stepped up their activity and they were very much smaller than we found them the previous February. But by this time we had a new set of stomachs to contrast with those we had been observing for a year, namely the stomachs of the incoming freshmen. These stomachs behaved precisely like those of freshmen of the previous year. They were very large and very sluggish, but in the course of the session they also cheered up, grew smaller and became more active.

In time it dawned on us that we were dealing with an organ very susceptible to emotional influence and that the apprehension and bewilderment of the freshmen at this new method, for new it was at that date, of learning anatomy, enhanced by the lurid suggestions of upper classmen, so played upon their stomachs that they froze with fright. We did not care to call it fright, so we dubbed it disquiet. When confidence has been restored to the subject of investigation his stomach movement is revived.

So far so good. We had interpreted satisfactorily the initial inactivity of the stomach, but we had still several years of study ahead before we realized the explanation of the large size. One of our collaborating students with a taste for mathematics, Dr. W. A. Sommerfield, de-

vised an ingenious method of estimating the volume of the stomach from the measurements of the x-ray shadow. By this quite reliable technique he was able to assure us that even though we gave only 4 ounces of fluid to a student on a stomach previously demonstrated empty, that stomach contained in less than five minutes not simply the fluid swallowed but an additional amount of fluid, which varied with the type of food swallowed. We had already noted that the stomach was much larger after giving 4 ounces of buttermilk than it was after giving 4 ounces of milk. Within 5 minutes after this quantity of milk is swallowed the stomach contains approximately 12 ounces of fluid, but after the same quantity of buttermilk it may contain 20 to 30 ounces. We then realized that gastric juice is secreted to an amount which varies with the type of food swallowed. But Dr. Sommerfield showed that these figures hold only for the stomach in a state of disquiet. The unruffled stomach does not contain nearly that amount of gastric juice. It is obviously preposterous that the stomach should secrete more juice if the subject is in an apprehensive frame of mind. Something must have occurred to bank up the secretion in the stomach. Hence we turned our attention to the pylorus or outlet of the stomach, the passage from stomach to duodenum or first part of the intestine, where there is a thick muscular belt surrounding the orifice. We watched this outlet narrowly and found that in disquiet stomachs it remains closed for a long time after food is swallowed, whereas in the stomachs of students well accustomed to the investigation it permits passage of contents within two minutes. We had now found the explanation for the large size of disquiet stomachs. The size of the organ depends less upon the amount of food taken than upon the relation between gastric juice secreted and the passage of contents through the pylorus.

Emotional states reduced the peristaltic activity and at the same time caused prolonged closure of the outlet.

If this interpretation be correct we should of course lose what we called the typical freshman gastric pattern as incoming students become more accustomed to the idea of being studied. This indeed is precisely what has happened. The freshman pattern of 1925 no longer exists in 1936. Freshmen of the current session have far more active stomachs than sophomores of 1926, and moreover at the very first examination we are able to demonstrate passage of contents through the pylorus within 2 minutes after swallowing food. But on the day of an examination or under conditions of mental stress the student's stomach once more relapses into the behavior pattern of the freshmen of 1925.

Our first lesson in the study of gastric behavior then is its susceptibility to disturbance of emotional origin. That disturbance is elicited by mental stress of any kind, by incipient illness, by hurry or by physical fatigue. The unwisdom of eating much before hurrying for a train, entering an examination, attending a board meeting where trouble is brewing, when feeling ill or physically exhausted is explained in objective terms. The pylorus closes. Contents are banked up in the stomach, the organ remains awash and the sensation of heaviness, distention and acid risings result from impaired motor function.

Some stomachs are much more prone than others to this disturbance of function, the chief site of which is to be found in the pylorus. Moreover, the disturbance is apt to ensue from certain specific foods. These act as a trigger mechanism, inducing closure of the pylorus in a sensitive stomach. If the reaction is very vigorous, vomiting will ensue as nature's method of relief. If, however, the response is less pronounced the victim continues for a considerable time in a state of gastric discomfort until the pylorus

opens of itself or is tricked into opening by the administration of soda or some other medicine like belladonna or amyl nitrite or by a drug which used regularly to form part of the gastric "sedative," namely, dilute prussic acid.

A victim of this disturbed gastric function experiences much more than mere distension. He may have pain and severe tenderness in the upper abdomen. Often he shows drowsiness, mental inefficiency and a tendency to abstraction or day-dreams. His temper may wear thin; he becomes irritable or he may have a prolonged bout of sneezing and congestion in the head. Curiously enough, the pylorus may be opened by a good sneezing fit. There is an excellent digestive in pepper or in the snuff-mull which our ancestors handed round with the port after dinner.

Vincent of Beauvais in 1250 A.D. extolled the virtues of cinnamon and pepper in curing disturbances of the stomach. It would be interesting to follow those irritable people of long ago who relieved their pent-up feelings and fulfilled their wild imagination by indulging in the spice wars to obtain those condiments so necessary for the comfort of their overworn stomachs.

Evidently we must glance at the reverse side of the picture, namely, the influence of the stomach on the mind. Samuel Johnson is a good example. His attack of nervous depression in 1766 was certainly of gastric origin. At any rate Boswell's "Life" abounds in references to the influence of the stomach, which was constantly and acutely bringing itself to the doctor's attention. There is no doubt at all of Johnson's chronic indigestion with which often goes a brilliance of imagery and creative thought. Benedick's "quick wit and queasy stomach" remind us of the indebtedness of both literature and science to indigestion. Would Darwin have framed the theory of evolution had it not been for the imagery created by his chronic indigestion? Would Conrad have written his stories had the facts of his experience not been sharpened and amplified by nervous dyspepsia? How much of Poe's tales of mystery and imagination were due to indigestion and how much to alcohol. It is so easy to ascribe to a drug the very talents which were in fact merely disordered by indulgence. For this attitude perhaps De Quincey's "Confessions" are most to blame. He thrust upon us the suggestion of association from which commentators and critics have never been able to free themselves.

Did Francis Thompson write "The Hound of Heaven" under the influence of opium or was it really his dyspepsia which "set the winds of inspiration blowing"? Could Coleridge's "Ancient Mariner" really be due to indulgence in "the milk of paradise" which distorts the mental vision or was it actually a vivid drama from a keen and orderly imagination?

There is a paragraph in the biography of George Crabbe, quoted by Abrams in his recent essay to confirm the theory that morphia was responsible for Crabbe's inspiration. After Crabbe had experienced a fainting spell in 1790 a certain Dr. Club was called, who "saw through the case with great judgment." "Let the digestive organs bear the whole blame; you must take opiates." Abrams may be right, but it seems to me far more reasonable to link Crabbe's genius with his constitution than with a poppy. The tumult and terrors of opium torment the imagination; they do not clarify it.

So often we impute to violence the authorship of the momentous things in life. Restlessness intrigues us, but restlessness is not progress. Somewhere in Horace there is a sentence which calls to mind the fact that those who cross the sea change only their climate and not their minds. True progress is measured not by pushing ever wider the boundaries of human effort but by deepening the fellowship of the spirit.

Perhaps it is pointless to pursue this argument. Better leave it incomplete

and return to the main theme. Certainly some folk sparkle with wit about the dinner table when others at the same meal drop off into somnolence, and one man's meat is another man's poison. One thing at least is certain. It is not from the torpidity of semi-poisoning or the disordered mentality induced by drugs that brilliance flows, whatever may take place in lucid intervals.

It took years of observation to make us realize that the phenomena we were observing in the activity of the stomach depend not alone on the state of mind but on the specific reaction of the stomach to what we put into it. Some people can not drink milk without disturbing their digestion. To others buttermilk is nauseating. There are those who dare not touch an egg, who get a rash from strawberries or break out in hives from what they call "over-heating" foods. Just what goes on in the stomach when a food to which one is sensitive is swallowed we do not know. But some mechanism is set in action which, as part of its manifestation, closes the pylorus firmly, just as emotion does. There is no doubt that the abdominal distention and discomfort which follow, in some people, the eating of certain foods, results from closure of the pylorus and banking up of contents which are greatly increased by the flow of gastric juice.

Of course this gastric juice really is elaborated from the blood, and fluid secreted in the stomach is actually withdrawn from the blood. Were there not a return flow to the blood stream by way of absorption from the bowel we should quickly become as dry as a mummy. But of this internal water circulation scarcely anything is yet known. By means of the x-ray we can watch the banking up of fluid in the stomach and its release when once again the pylorus is opened to permit its passage.

So now we come to the question of what makes stomach contents pass into the duodenum. It is a rather arresting fact that, whereas saliva flows when we chew solid food but not when liquids are taken, gastric juice can be seen to pour into the stomach far more freely when liquid nourishment is swallowed. The device is obviously a flushing of the fluid through the stomach by the outpouring gastric juice. When solid food is swallowed peristalsis does act to some extent in passing it onwards, though the relatively small amount of gastric juice then secreted plays its part as a flushing agent. Whatever the food swallowed, liquid or solid, provided there is no emotional barrier and the stomach is not "sensitive" to the particular food swallowed, passage into the duodenum can be observed to commence within two minutes. It is certain then that the long periods during which some foods are known to remain in the stomach merely indicate a sensitivity in that person to that food. None of the 800 medical students whom we have investigated, unless his stomach has this disability, has ever been able to command a breakfast or a lunch which remains in the stomach more than an hour and a half. The popular notion that some foods remain longer in the stomach than others must be based, not on the healthy activity of the stomach, but upon a disordered function due either to emotional influence or constitutional sensitivity registered in advance in dislike or in retrospect in discomfort.

The conditions of our daily life formulate for us, did we but know it, the limitations of our dietary and the artistry of its application. For the stomach is a self-respecting organ which resents rough handling and manifests its disapproval by local signs. If these are disregarded it dislocates the mechanism of the body, touches the mind and casts dismay into the spirit. The stomach may well be characterized as the organ of social adjustment. In it are reflected, as from it flow, the social blights or blessings of the time.

YOU CAN'T TRAIN THE INTELLECT BEFORE IT ARRIVES

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I

Of all the psychological gold bricks ever foisted upon a gullible public, there are few that can match the one which claims to make your baby into an intellectual giant by the right sort of training administered in the cradle. Here is the claim, or at least a part of it, presented as a study in black and white.

> There is no such thing as inheritance of capacity, talent, temperament, mental constitution and characteristics. These things depend on training that goes on mainly in the cradle. . . . Give me a dozen healthy infants, well-formed, and my own specified world to bring them up in, and I'll guarantee to take any one at random and train him to become any type of specialist I may select—a doctor, lawyer, artist, merchant-chief and, yes even beggar-man and thief, regardless of his talents, penchants, tendencies, abilities, vocations and race of his ancestors.

There would be millions in it in tuition fees for any one who could fulfil this promise, but one hears of no stampede to open the necessary nursery schools to turn out the product. There might, however, be something less desirable than millions as the net result if the claim turned out to yield fool's gold rather than the genuine article. The claim appears to be widely accepted outside of professional educational circles, but this acceptance seems to be for academic and theoretical purposes only. It is possible, too, that there is, tucked away in the inner recesses of the unconscious, a complex left over from earlier mental conflicts, to the effect that all is not gold that glitters. And this seems to be effective even in spite of the wishful thinking which might tend to create an attitude favorable to acceptance of the alluring claim; for what fond dad is there but is willing to believe his very own infant capable of attaining to anything from Caruso to President, if only supplied with the opportunity that, unfortunately for the rest of the world, was denied to his dad?

II

But what problem is there which should be of greater concern than the possibilities of human development? And why should not at least as much care and consideration be given to it as to the possibilities of improving automobile construction? As a parent one is and ought to be concerned as to the means by which his children may develop to best advantage. What are the possibilities at each stage of development—infancy, childhood, adolescence, adulthood? What are the phases of development which are attainable at successive ages? What are the limits, if any, to the mentality, the personal and social assets which may ultimately be developed by any given individual? These are questions worthy of the most careful investigation by the most competent scientists available.

For the professional educator, whether in the capacity of teacher, superintendent, university professor or United States Commissioner of Education, the problems of possibilities and limitations of learning and development loom large in significance. One's beliefs on such things largely shape his educational policies, and therefore it is important that those beliefs should be founded upon fact and not upon wishful thinking nor upon unwarranted claims.

Nor is the situation different in the

case of the would-be philanthropist who seeks to lessen the human distress and the tremendous economic waste involved as direct and indirect results of feeble-mindedness, degeneracy and crime. What can be done about it? Will slum clearance reform a community? Is it the physical structure which determines the quality of the home, or is it the character of the people in it? Will improved educational facilities abolish crime and dependency in normal times? These problems are of vital significance to all parties concerned.

When an influential educator confronts a superior group of young people, how tempting it is for him to claim them as the product of the schools whose interests he is fostering. But how about the morons and the delinquents and the incurably shiftless who are also the product of the same educational system? One could wish that every influential citizen were familiar with the outstanding problem which confronts our schools—the problem of individual differences. Visit a class of the intellectually subnormal, and then visit one of the major-work classes for the intellectually gifted in our city schools, and you will get a picture that will not soon be forgotten. The contrast to the uninitiated is astounding. Nor does the matter become any simpler when one realizes that some of the most capable principals and many of the most competent teachers are to be found in the schools where the educational product is the most deplorable. The fact of the matter is that the educational facilities in a large cosmopolitan city, in the way of equipment and teacher personnel, do not vary greatly from one school to another. They play no favorites in a large city school system, but provide as best they may the educational opportunities most suitable to all. In fact, until recent years the schools attempted unwisely to provide *the same* opportunities for all.

When the farmer has drained the swampy places in a field and fertilized the barren spots till all parts are nearly on a par in productivity as a result of improved nurture, it is not variation in the cultivation that is responsible for the wide range of the product—potatoes, cabbage, ragweed, little wild strawberries, large luscious strawberries, maple trees, red roses, dandelions or what not. One does not grow figs from thistles. A horseman does not attempt to produce a race horse from a Clydesdale colt by any miracle of early training. He believes that whatever of training skill he has will be expended to better advantage if he selects a colt of race-horse heredity upon which to expend it.

And if the farmer wishes to restrict his planting to one species of product, say strawberries, he gives careful attention to the matter of heredity in his planting. He does not plant any and all vines that happen to be strawberry vines. On the contrary, he practices eugenic principles shamelessly. He eliminates the poor little sour strawberries altogether and plants only those vines which have the finest heredity. On this basis he predicts large, fine-flavored berries with comparatively little variation in the product, and he gets them. He believes in having his fruit well born. He is not sentimental about it, because it is the quality of the product with which he is concerned. He does not save even a random sampling of the poor little sour strawberries. With the quality of the soil remaining constant, and the cultivation unchanged, he has greatly restricted the range of his product and improved the quality permanently, merely by giving attention to the heredity factor—the seed planted.

III

One who studies the situation in the schools of one of the large cities will find a greater range of product in a single school than could be found on a farm where no attention was given to selection of the seed to be planted. Some farmers still grow many small potatoes as well as

large ones, and it takes more small potatoes to make a bushel than if the potatoes are large. But how many morons does it take to make a genius? Rather, what proportion of the energies of the genius must be spent to help provide for the moron? Morons as a class are a heavy liability. It is doubtless true, however, that the large majority of people, including some in professional circles, accept in theory the belief that it is the nature of the environment which largely determines the kind of person produced. This view is the one which fits in well with convenient interpretations of democracy. It is well voiced by Wat Tyler, who led the Peasants' Revolt in the reign of Richard II in England:

> When Adam delved and Eve span,
> Who then was the gentleman?

People who were born into a feudal system of society were seriously and unjustly handicapped thereby. Those who have no opportunity to learn to read or to write are largely shut out from contact with the intellectual inheritance of the ages. They are seriously curtailed in the development of the hereditary abilities which were potentially theirs. But that is not the story here under consideration. There are not to-day the wide differences in educational opportunity which characterized the Middle Ages. In a large modern city, elementary and secondary schooling are provided for all. Indeed the law requires all to attend school until they are sixteen or eighteen, so that each may get whatever his abilities enable him to get.

But the phase of Tyler's rhyme which is of interest in this connection is its implication that descendants of the same remote ancestors, members of the same species, are equal in their possibilities of learning and development. I am reminded of the moron who gave a welcoming hand to his former schoolmate in the village school, who had just returned home as a newly graduated M.D. "Jim, ye looks swell," grinned the moron. "I wish now I'd 'a stuck to school like you done. Jist see where I'd 'a been to-day 'stead 'a hoein' turnips."

In my judgment there is no implication in the whole realm of educational theory which is more fundamental than this. If one infant can be educated to become as able and as worthy a citizen as any other infant, then why under heaven do we tolerate an educational system which regularly develops one or two per cent. into morons or lower, one or two per cent. into highly gifted intellects, and the rest into varying degrees of mediocrity? Why does this distribution of human abilities correspond so closely to a normal probability curve, to which so many things in the biological world conform in their distribution of individual differences? If the production of Newtons and Edisons and George Washingtons depends on training which goes on mainly in the cradle, then why doesn't some one bring back the cradle! Most experienced teachers of unselected school children know the answer to this question as a result of their experience. Experts in educational psychology know the answer given by well-controlled scientific experiments carried out within the past few years. The evidence is there for those who have the time and the energy to dig it out. We shall come to it presently, but first the historical background of the commonly accepted view deserves consideration.

IV

Whatever may have been the influence of such men as Wat Tyler, Rousseau and would-be philanthropists from time to time who have tried to eliminate the criminal, the moron or the irresponsible incompetent by some utopian plan of education, it was the English philosopher of the seventeenth century, John Locke, who was mostly influential for at least two centuries in shaping the opinion in the universities with regard to the value

of mental training. Locke took what seemed superficially to be self-evident, made it articulate and gave it the weight of scholastic authority. Who has not noticed that the right arm of the blacksmith becomes strong by exercise? What observant man of the eighteenth or nineteenth century could fail to see that it was, for the most part at least, the men who took classics and mathematics in the universities who became the most accomplished intellects of the age? Was it not the exercise required in mastering those difficult subjects which made them intellectually strong? Up to the beginning of the present century it was unquestioned educational theory that it was the mental stunts practiced in college which increased the power of the mind as a whole, or increased the power of more specific faculties of the mind such as the memory, the judgment, the reasoning powers and the imagination.

But at the beginning of the present century, this disciplinary theory of education received a shock at the hands of a pair of young psychologists from which it never recovered. When tested experimentally by Thorndike and Woodworth in 1901, it was found that mental exercise did not improve the mind as a whole, nor even the more specific faculties in any such wholesale fashion as the old disciplinary theory of education had assumed. A new educational psychology was in the making. Mentality was put into the crucible. The battle waxed hot. But all along the line opinion and the armchair philosophy of the old school had to give way before the experimental evidence presented by the new school, which demanded the right to examine the basis for the assumptions of the past. Things began to happen in several different fields closely related to educational theory and practice. The school administrator began to press seriously for an accounting of the expenditure of school funds in terms of educational product. Why were many children retarded a year, others two years and still others three or more years, in the very same schools in which other children did the expected requirement of one grade per year without loss of time? The waste of the taxpayers' money as a result of retardation in the schools was put into the limelight and laid upon the doorstep of the educational psychologist for solution. Why shouldn't all progress at the same rate when they had the same teachers?

The problem was just as insistent in the schools of Paris as it was in the schools of American cities and, as it happened, Alfred Binet, working under the stimulation of the school board of the city of Paris, was the first to offer an answer that was reasonably convincing. His means of demonstration was the Binet scale for the objective measurement of intelligence. His answer was that children vary enormously in their inherent capacities for learning and that such differences must largely determine the rate of learning to be expected. Moreover, it appeared that individual differences in ability to learn were highly stable as well as wide in range and that they must be taken into account in all educational policy.

Meanwhile Terman, Thorndike, Norsworthy and others had been working on the same problem, and were precisely in line with the findings of Binet, and all set to profit very rapidly from his stroke of genius in putting the results of many years of experimentation with intelligence testing into workable form. Under the leadership of Thorndike, tests for the objective measurement of school achievement began to be constructed in accordance with the best-known statistical principles. Once the pattern was set and the matter given national publicity, the construction and use of such tests progressed by leaps and bounds. Experimentation in school problems by the aid of these new tools for measurement of mental

ability and of school achievement proceeded with surprising rapidity. In fact, the growth was somewhat too rapid for the good of the cause, although the leaders made every effort to put on the brakes and to keep the movement within due bounds. By and large, however, the general upshot has been the most notable advance within the last quarter of a century that has ever been recorded in the history of education in a like period of time. The advancement, particularly in the last five years, has been tremendous, and it has included very important work on the heredity-environment problem.

V

It took a Darwin to make it clear that man is an animal in every sense of the word and, as such, subject to the same biological laws of heredity and environment as other forms of plant and animal life. The first man of genius to investigate the laws of inheritance in plants was an Austrian monk named Mendel. Mendelian principles of heredity remained practically unnoticed for three decades, but finally came into their own about 1900. The science of genetics, which seeks to unravel the laws of inheritance in plants and animals, has developed with great rapidity, especially since 1915. It is impossible to summarize the findings of a whole new science in a few words. However, some of the incontestable facts brought to light are that the wide range and the distribution of the differences found within a species under ordinary conditions are to be explained in terms of differences in heredity—differences in the chemical packets called chromosomes which are contained in the fertilized seed or egg from which the plant or animal developed. These chromosomes are paired, one member of the pair always being supplied by the female egg-cell, and the other member by the male sperm-cell which fertilized it. The male sperm-cell in man is so small that it requires a high-powered microscope to see it. It is made up almost exclusively of twenty-four chromosomes. One of these chromosomes contains the factor which determines the sex of the individual to be. The progeny on the whole resembles the father as much as it does the mother. And yet this sperm-cell, microscopic as it is in size, is absolutely unique in the chemical packets it contains. It has thrown away exactly half of those which made up the inherited constitution of the father, and has kept a combination as unique as the hand one receives in a deal in bridge. Moreover, the resulting combination of chromosomes is the result of a chance shuffle, just as the hand of cards is due to chance arrangement determined by the shuffling of the pack of cards. The same is true of the egg-cell which likewise becomes half of the inherited constitution of the new individual.

Thus genetics explains why it is that two brothers never have the same hereditary constitution and why so-called "identical twins" do have the same or nearly the same hereditary constitution. Identical twins are descended from a single fertilized egg which has split into two, while two brothers are descended from two different eggs, each having its own individual combination of chromosomes. On the whole, children resemble their parents to an extent indicated by a coefficient of .50, where a coefficient of one would represent identity, and a coefficient of zero would represent no greater likeness than in people of no blood relationship.

The significance of all this for educational theory lies in the fact that individual differences in inherent nature are to be expected, even in the case of children of the same parents, and that incomparably greater differences are to be expected in the children of parents who vary widely in hereditary endowment. With minor exceptions due to special causes such as injury occasioned at birth,

the difference between the genius and the moron is to be accounted for in the main by differences in hereditary constitution.

VI

But meanwhile there have been other influences on the educational horizon which have made the situation difficult to evaluate, particularly for the non-professional. First came Freud with his cases of emotional complex which he explained as the result of early experiences, often long since forgotten in the ordinary sense of the word. And if such experiences upon investigation turned out to be inadequate to explain the mental disorder, Freud was not disconcerted thereby. On the contrary, he merely sought the causes in still earlier experiences in babyhood or infancy. One sometimes suspects a case of retreating to still more inaccessible strongholds as the original ramparts become untenable. One is reminded in this connection of the duckling that was inadvertently hatched by a hen along with a brood of chicks. Everything went well until one day in their wanderings the hen and her brood encountered a pond. The chicks behaved as properly reared chicks should, but the duckling was so delinquent as to take immediately to the water, thereby causing the disconcerted foster-mother no end of concern for the unbecoming conduct of her foster offspring. One suspects that the environment often gets the blame for a lot of things which can hardly be charged exclusively to its account. Who knows but that the pond, instead of being the cause of the duckling's downfall, was in reality the lucky chance that saved the poor duckling from heading directly for a nervous breakdown, which would ultimately have required the services of a duckling psychiatrist? At any rate, many will doubtless prefer to think that original nature had something to do with the fact that the same environment which led to the downfall of the duckling in the eyes of the foster hen, produced no such effect upon the chicks. Both Freud and Watson, in their cocksure pronouncements about the influence of early experiences, might well weigh the matter of individual differences in inherited constitution, as illustrated by the matter of the duckling and the pond.

Before the opening of the present century, Thorndike had achieved notable results in the study of learning expressed in objective terms in conformity with his well-known S – R formula, later used to good advantage by Pavlov. Thorndike always emphasized the fact that it was a highly complex *organism* which received the *stimulus* and that the nature of the organism always had much to do with the character of the *response*. The implied formula was S – O – R, with the "O" written large. But in the hands of Watson at a later date, the stimulus became the all-important factor. The organism was forgotten, and from his point of view it was the stimulus, that is the environment, which was responsible for the behavior which ensued. By 1917 Watson began to attract wide attention as a result of the experiments he had performed on the emotional learning of babies in a maternity hospital. He found that native responses were almost non-existent in the infant, and that it could easily be taught to exhibit the fear response to objects originally attractive in character. Watson interpreted these experiments as indicating that the infant is capable of rapid learning, that he can be shaped in any way desired by early training and that "There is no such thing as inheritance of capacity, talent, temperament, mental constitution and characteristics. These things depend on training that goes on mainly in the cradle."

Unfortunately, Watson soon ceased to be a university professor and became a professional advertiser, and his conclusions began to spread far beyond his data.

Also, unfortunately, his experiments on babies fell far short of meeting ordinary scientific standards.

Watson's general arguments in support of the potency of environment to the exclusion of the hereditary factor may be dismissed with very brief comment. As for the suggested view-point of the anthropologist to the effect that man's nature and inherent capacity were about the same "many millions of years ago" as they are to-day, and the implication that the differences between primitive man and civilized man are due to differences in the environment, one may well raise the question as to the nature and reliability of the evidence that man's nature and mental ability were the same many millions of years ago as to-day. If one skull is found of the millions of primitive men of a given age, what statistician will assure us that the one will be a reliable average of them all? And even if it were a reliable average, what psychologist will venture to give us a reliable measure of the intellectual capacity of such an individual on the basis of the measurements of his skull? Such arguments are hardly the kind of thing one would expect from an ultra-scientific experimental psychologist.

As for the sociological argument that a favorable environment may change a group or a nation to a cultural level far beyond that previously attained, the educator can agree most heartily. Surely the educator may be expected to believe in the potency of a favorable environment when that is what he makes it his professional business to supply. But this is beside the point. It does not touch the perplexing problem of individual differences at all. The improvement of the general average of a race as a whole does not bring about equal improvement for every member of the race or group. The moron is still with us in the finest educational system that any one has been able to devise. In fact, the intellectual differences between individuals become still wider as the educational opportunity is improved for all. The idiot remains an idiot, no matter what the environment in which he is reared, whereas the genius is able to capitalize fully upon his learning capacity only under conditions most favorable to the full exercise of his superb learning capacity. The damage is done by the implication that all infants are alike.

But the part of Watson's contention which requires most critical consideration is the experimental evidence which he claims to have secured in support of his conclusions. Following out the suggestion of Thorndike made in 1898 that "More fruitful than a host of questionnaires would be a set of experiments on children from birth on, in circumstances where their experience of, and instruction about things, could be regulated and its influence as a disturbing factor completely known," Watson nearly two decades later undertook to investigate the unlearned behavior of infants, and the aptitude of one or more of them to learn and unlearn fear responses. The infant exhibited the fear response well enough. Most any animal will do that. And wild animals have to be able to switch that response to different objects readily enough too if their chances of survival as individuals are to be worth much. Rats quickly learn to fear a trap that goes off with a zip and a bang. They don't need to be highly intellectual to do that. The shock will do the trick. It may be that the neural apparatus brought into action is lower down than the precious gray matter which the highbrow is supposed to employ. At any rate such learning is not necessarily an indication of future ability to learn calculus, provided behavioristic training is given in the cradle. In fact, one trouble with many humans is that they are too apt at learning fears, most of which are highly irrational. And all too frequently these

block the way to the intellectual learning that the educational fraternity would like to see.

Watson was right enough in noting that here was learning that needed no mind, or at least a minimum of mind. If only he had realized that there might be different kinds of learning! For in some respects the learning of the chick or the rat seems to be different from the learning hopefully looked for in the classroom. One of the cardinal principles of scientific procedure, however, is to limit one's conclusions to the data under consideration. Immature minds tend to jump at conclusions. What a pity that Watson did not caution his readers that he was dealing with the most primitive form of emotional response and that this behavior might have little relaton to "capacity, talent, mental constitution and characteristics"!

Now as to Watson's conclusion that there is no such thing as inheritance of capacity because he finds little in the way of unlearned behavior at birth. Does it therefore follow that everything characteristic of adult behavior must be explained exclusively by learning? On this basis Spalding, working in 1875, would have been justified in denying that capacity for flight in birds is inherited. There was certainly no such capacity in evidence when the birds were hatched, and on the other hand ordinary observation gave every indication of gradual learning to fly on the part of the fledglings. But even at a date which is credited with being about four years in advance of the birthday of psychology as an experimental science, Spalding was too good a scientist to jump to such a conclusion without actual testing of other possible explanations. He eliminated the possibility of learning to fly by keeping a brood of swallows in cages too small for them to lift their wings, and then released them when the appropriate stage of maturation had been reached. They flew unaided, but there were marked individual differences in the effectiveness of their first attempts at flight. Some flew off with a sudden and amazing skill, one in particular flying right off out of sight, never to be seen again.

These experiments were repeated with different broods, and later repeated on three different species of birds. They proved conclusively that flight in birds must wait for the maturation of the motor and neural structures needed in flight, and that when that point is reached learning is unnecessary to explain a very creditable degree of performance. If this is what is meant by instinctive behavior, then birds such as these can fly instinctively. In other words, some capacity for such behavior is inherited. Moreover, when there is something there with which to practice as a result of maturation of inherited capacities, learning may proceed from that point.

In spite of much emphasis upon both maturation and individual differences in relation to learning on the part of later experimenters, Watson in his experiments with infants entirely ignored the possible influence of both. Many experiments, especially within the past few years, have demonstrated beyond doubt that a great deal of early behavior which superficial observation is prone to attribute to learning, turns out to have been almost or entirely uninfluenced by such "learning." Even in cases where the learning seems at first to be effective, the effect turns out to be superficial and temporary, with no permanent improvement in the capacity exercised at this premature period. For example, Gates, experimenting with carefully matched groups of five-year-old children, has shown that continued training in memory span over a period of five months resulted in marked improvement, but that the improvement had entirely disappeared four and a half months later. There was no

evidence that the continued practice had increased any fundamental capacities. The control group which had had no training did just as well as the specially trained group. The same has been found to be true of motor functions, of linguistic development and of mental development in general by Gesell, Strayer, Hilgard, Goodenough and many others. The essential point seems to be that one can not develop an ability which is not yet there. To do so smacks too much of creation, and creation is for the almighty or the near-almighty. That is precisely the difficulty under which one labors in trying to educate an idiot. There is nothing there to work on. Maturation has had its chance, and nothing worth speaking of has materialized.

One can agree with Watson readily enough that there isn't much if anything there in the way of inherited capacity of an intellectual sort in early infancy. And again it is a pity that he did not have the patience to wait and test again at a later date. The development of the human organism has its beginning some nine months before the event of birth, and continues to mature for possibly as much as two decades after birth, depending upon the race and the individual. Why should birth be singled out as a time when hereditary factors might be assumed to cease to operate all of a sudden in shaping development, especially in view of the fact that birth takes place at variable ages and stages of development?

Now to come to the matter of Watson's discovery of the lack of individual differences in intelligence in early infancy. The fact itself is not to be denied. With nothing in the way of intellectual capacity yet discernible in any infant, one could hardly hope to find individual differences therein! But what is the approved technique for determining individual differences in intelligence? The only means relied upon by professionals is the use of a standardized scale for the measurement of intellectual capacity. But Watson, being an animal psychologist and not an educational psychologist, was possibly unaware of the need of such a tool for this purpose, or of the fact that there is no such test available for use in early infancy, for the good and sufficient reason that there is practically no mental behavior there to be tested at so immature an age. What behavior there is is almost entirely sensory-motor in character rather than intellectual.

To consider the positive side of the picture, what evidence can any one produce of intellectual training given in the cradle period that ever produced any permanent intellectual development? Much rhetoric has been expended in an attempt to prove the plasticity of infancy, but little if anything has been advanced in the way of evidence in support of this glamorous claim. If plasticity means modifiability or ability to learn, then early infancy would seem to be about the most unfavorable time of all, at least in the way of intellectual learning. Who is there among my readers, either of the gentler or of the more masterful sort, who will undertake to develop in the infant anything of intellectual significance during say the first three weeks after birth or even the first three months? To begin with, the infant is asleep a good deal of the time, though this should not greatly interfere with behavioristic learning which is independent of consciousness anyway. Certainly one can not teach the infant to talk at that age. How then is one to communicate any information? Surely this fantasy of prodigious learning which goes on mainly in the cradle must be relegated to the attic where most cradles are now to be located, if indeed they are not already in the discard. Experimental evidence all along the line has brought to light more and more the barriers to most kinds of learning at the earliest stages, and the gradually increasing ca-

pacity for learning up to age eighteen or beyond, rather than increasing capacity for learning as one approaches the date of birth.

VII

In view then of the assertions of Freud and of Watson from the outside of the educational circle that early experiences and training account for the differences in people, and of the persistent and rapidly accumulating evidence of the striking stability of inherited differences in intellectual capacity on the part of professional workers within the educational circle, is it any wonder that Terman in 1924 issued his educational broadcast calling for competent investigators everywhere to get down to business in an effort to solve the nature-nurture problem by a crusade of scientific exploration? The answer to the call materialized in February, 1928, as the two-volume Yearbook of the National Society for the Study of Education. The thirty-nine experimental studies and discussions finally accepted for publication in the Yearbook were restricted to just two considerations: "Nature and Nurture, Their Influence upon Intelligence," which constituted Volume I, and "Nature and Nurture, Their Influence upon Achievement," which constituted Volume II. No serious student of educational policy can afford to remain in ignorance of the essential contributions contained in these two volumes. Many of the experimental studies reported are highly technical and not to be mastered without careful study. However, the two most extensive studies, the California study sponsored by Terman and the Chicago study sponsored by Freeman, have to do with the effects of training given to adopted children. They remind me of a case which came under my observation some years ago. A philanthropist of considerable means, with whom I was well acquainted, adopted an infant son. The mother had died and the father was a good-natured, sociable but irresponsible ne'er-do-well with alcoholic tendencies. The philanthropist had a son of his own, a year or two older than the adopted son. Both boys in due time attended the same private school. But by the time the adopted son had reached the seventh grade, it became apparent that little was to be gained by keeping him at school any longer. By the time he was sixteen, he was detected taking small sums of money which did not belong to him, and using the money to treat his cronies to smokes and sweets. In temperament, intellectual capacity, sociability and even in manner of gait he was strikingly like his natural father, whom he had seldom seen. He later enlisted in the navy, while the natural son went on to college.

Here was an instance of two boys of very different heredity, both of whom from early days had been favored with essentially the same educational and social opportunities, and yet in spite of the similar if not identical environment, both developed largely in accordance with their own inherited natures, like the contrasting varieties of strawberry raised in the same field. Cultivation undoubtedly did something for both boys, but much more for the one than it was able to do for the other. Of course a single case such as this is in itself unconvincing. It may not be representative, and it lacks scientific rigor. It is merely suggestive and illustrative. The matter is put to the test of experiment in the California and the Chicago studies. Here the attempt is made to estimate or to measure the rise in the intelligence of adopted children when reared in superior homes, and the degree of resemblance of adopted children to their foster parents, as compared with the resemblance of true children to their own parents. The two studies agree quite well in part. They find that the rise in I.Q. as a result of being brought up in a superior foster home is limited to about

six to nine points. As to the remaining data the studies diverge considerably in interpretation. The California study finds about 17 per cent. of the variation which takes place in intelligence under ordinary circumstances is due to the environmental factor, while 75 or 80 per cent. is due to the hereditary factor. The Chicago investigators, on the other hand, interpret their data as indicating that the environmental factor appears to be a decidedly potent one in determining intellectual development, or at least that it is by no means a factor of minor importance.

However, of all the studies reported in the Yearbook, the Chicago experiment stands out as the striking exception in this conclusion. The general conclusions may be summarized in part in the words of the editor of the yearbooks, who is also the permanent secretary of the society, as follows:

> Various human traits do not behave alike, and the relative influence of heredity and environment is something to be determined independently for each of the numerous traits that we strive to influence in the home and in the school.
>
> No one who reads the yearbook can put it away with the conviction that general intelligence is an absolutely fixed, immutable, innate capacity, but neither can one put it away with the conviction that general intelligence is readily susceptible to environmental influences.
>
> It is my impression that the data assembled emphasize the preponderant role of intelligence in conditioning school achievement. The Twenty-Seventh Yearbook strengthens the contentions of those who, speaking in hyperbole, are termed 'hereditarians.' The Yearbook so far as I can perceive, affords not a single straw to be clutched at by those extreme environmentalists who deny in toto the inheritance of intellectual disposition, and who would have us educational workers accept the staggering doctrine that, if we only knew our business, we could make of any child whatever we wished, even to fashioning of the proverbial silk purse from the sow's ear.

More recent investigations, notably those of Leahy at the University of Minnesota and that of Lawrence in England strongly support the conclusions of the California study. In short, recent data indicate in quite convincing fashion that the *inherent intellectual capacity* of an individual can not be very greatly increased by any known means of mental training at any period of mental development, and that under present educational conditions, the differences in ability and in achievement between school children are due to a far greater extent to differences in inherited nature than to differences in environment or in educational opportunity and training.

There are some indications that differences in personality traits may also be due to a considerably greater extent to inherited nature than to common differences in training, though the data on this point do not warrant any definite conclusion. Yet there is probably enough to make it advisable to suggest to the criminologist who is sure that an unfavorable environment is the whole cause of the career of the criminal that it would be advisable for him to recall the episode of the duckling which took to the pond, and to investigate the possibility of the inherited factor existing as a potential cause of at least certain types of crime, before being too sure that superficial indications tell the whole story. Certain it is that present theories of crime and methods of treatment based thereon can hardly be enthusiastically supported on the basis of their success in securing desired results.

VIII

One wonders how long it will be before those who behold the wonders that have been performed in plant cultivation and in animal breeding by more or less wholesale application of the principles of eugenics, will continue to stand aghast at the very thought of considering their possible application in human affairs in even the slightest degree. One wonders how long public lecturers, who readily admit the vast improvement which has taken place in stock raising by elimination of the most unfit by selective breed-

ing, will continue to argue that it would be out of the question to improve the human race by the application of eugenic principles anyway, because one does not know which are the best human beings and which are the poorest! One might at least venture to hazard a guess that there are a few specimens whose pattern might be dispensed with without too much danger of loss on the part of future generations; and on the other hand that there are many whose progeny would be a very good bet to stand well above the average in many desirable traits. However, there is a ray of hope in the fact that public attitudes on other considerations have sometimes changed almost over night, as for example on the matter of woman suffrage.

In the last fifty years, a large part of the possible improvement of intellect by improvement of public educational facilities has already been accomplished. We are like the farmer who has drained the low spots on his farm, and fertilized the barren places, and cultivated the whole farm to the best of his ability, but still continues to plant any old seed with never a thought as to whether he might now turn his attention to improvement of his product by the simple expedient of more judicious selection of the seed to be planted, to the point at least of declining to plant seeds most likely to prove harmful, or of favoring the selection of seed most likely to give rise to the more desirable products—assuming of course that the human product is worthy of at least as much consideration as strawberries or cattle.

It goes against the grain to believe that men are not born equal, but it is true beyond doubt. Neither is it indicative of abnormality for an individual to have potential ability much above the average or much below the average. It is not abnormal to get a hand of bridge with a ten spot high occasionally, nor one with at least three aces. Just such combinations may happen in the distributions of chromosomes, even when the chromosomes from which the selection is made are the same, as is true in the case of children of the same parents. Such combinations are the result of natural forces just as much as are the combinations which occur more frequently, but it does not follow that the most unfortunate combinations need to be perpetuated eternally just because they occur, nor that the world's greatest assets deserve no thought toward their perpetuation.

It goes against the grain to have the outsider, the back-seat driver, tell one exactly how matters stand and what ought to be done about it. It takes persistent and usually thankless effort to get people away from accepting what has little or nothing to support it but wishful thinking. It is difficult to acquaint the general public with the facts laboriously brought to light by painstaking scientific research. How effective by way of comparison are the methods of the popular advertiser! And yet in the long run it is well worth the effort, for what but the truth may be relied upon to guide human effort so that the labor of millions may not be spent for that which is vain.

THE PHYSICIST'S NEW DELUSION

By Dr. MAX SCHOEN

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It is a characteristic of human experience that one extreme generates another extreme, a good instance of which is to be found in current physics. The physicist at one time harbored the delusion of possessing absolute knowledge, and when that delusion was destroyed for him he sought refuge in the opposite delusion that he knew nothing, but sought refuge from that unpleasant delusion in another delusion, that because he knew nothing in physics he must know everything or a good deal in extra-physical matters like philosophy and particularly religion. This is not implying that all physicists have gone off on that tangent, but it is a significant symptom of general conditions that those whose writings about current physics have attained the widest popular appeal have let loose a stream of speculations in the name of physics which is greeted with enthusiastic approval by a band which at one time shouted itself hoarse in denunciation of science but which hails it now as its strongest ally and friend. What has happened? Has physics lost its scientific status or has its historical enemy given up its dogmatisms? The answer is obvious, namely, that the dogmatic theologian has not come over to science, since he proclaims that science now supports his historical position, and therefore it must be that some scientists are giving him good cause for reaffirming his dogmatisms. Now when we recall what the scientist has suffered at the hands of the ecclesiastical dogmatist it is a betrayal of science and the scientists of the ages to give him any comfort and an opportunity to point his finger in derision and say "I told you so." The obscurantist and obstructionist is ever with us, whether openly or in disguise, and if he is given an inch he'll claim a mile. We should be jealous of science when we consider its uphill fight in the course of the centuries and when we bear in mind that every positive material good that mankind enjoys to-day has come from the scientific laboratory, whereas all evil that is still with us is due to traditional dogmatism obstructing the introduction of the scientific mentality into social affairs. Any comfort given to the enemy of science, though he may parade as its friend, is a betrayal of man simply because it is a betrayal of knowledge, and there is no knowledge that can benefit man other than the knowledge we call rational science.

But does not the physicist have the right to speculate? The answer is that when he speaks in the name of physics he has no right to speculate beyond physics, and when he does speculate beyond physics he should make it clear that he is not speaking as a physicist, and he should also make certain that he has the discipline for speculation in that extra-physical sphere. Now, speculators in the realm of physics like Eddington, Jeans, Millikan and Arthur Compton are guilty of two charges. They speculate in extra-physical spheres in the name of physics, and the quality of their speculations indicates their lack of discipline in those spheres. In other words, they go beyond physics in their physical speculations and they are naive in the method of their speculations.

Let us take an instance from Eddington. In one of his books Eddington is engaged in demonstrating the mystery of the universe as revealed by current physics, which leads him to proclaim the

reality of religion also in the name of physics. One of his arguments runs as follows:

> If, for example, we admit that every thought in the mind is represented in the brain by a characteristic configuration of atoms, then if natural law determines the way in which the configuration of atoms succeed one another it will simultaneously determine the way in which thoughts succeed one another in the brain. Now the thought '7 times 9' in a boy's mind is not seldom succeeded by the thought of '65.' What has gone wrong? In the intervening moments of cogitation everything has proceeded by natural laws which are unbreakable. Nevertheless we insist that something has gone wrong. However closely we may associate thought with the physical machinery of the brain, the connection is dropped as irrelevant as soon as we consider the fundamental property of thought—that it may be correct or incorrect. The machinery cannot be anything but correct. We say that the brain which produces '7 times 9 are 63' is better than the brain which produces '7 times 9 are 65'; but it is not as a servant of natural law that it is better. Our approval of the first brain has no connection with natural law; it is determined by the type of thought which it produces, and that involves recognizing a domain of other type of law—law which ought to be kept, but may be broken. Dismiss the idea that natural law can swallow up religion; it cannot even tackle the multiplication table.

Now here we have obscurantism of the most opaque type parading as physics, and playing right into the hands of the enemies of science. That every statement here made is a misstatement, revealing the most deplorable ignorance of the nature of the learning process and neural activity, and that it is a mockery of God and religion to deduce both from a boy's ignorance of the multiplication table, is beside the point. The important point is that here is a man with a reputation as a scientist talking in extra-scientific realms in the name of science in a most deplorably unscientific manner. What is wrong? The answer is that in his philosophical and religious speculations Eddington and the rest are neither scientists nor philosophers nor religious thinkers. They are purely theologians, and poor ones at that. The demonstration of this thesis calls for a definition of science, philosophy and religion.

Science is a method of procedure, a way of thinking, that, judged by its results, offers the greatest promise to produce knowledge that enables us to deal most effectively or intelligently with our world. The factors that make this procedure intelligent are at least four in number. First and foremost scientific thinking draws a clear-cut distinction between what *can* be believed because of the reliable evidences or data on which it is based, and what *is* believed on the grounds of feeling, temperament, tradition or prejudice. This is a factor of intelligence simply because to mistake what is believed for what can be believed means harboring a delusion. When any person feels that what he likes to believe is also what he can believe he is taking the first steps to a lunatic asylum. There are cases of course when that which is believed turns out to be also that which can be believed, but in all such cases that which is believed has been tested out by that which can be believed and found to be valid.

The second principle of scientific thought follows from the first, namely, that the evidences for that which can be believed must be beyond question, which means that they must be impersonal, public or universal data, or, in other words, that they are objective, measurable and causal. These three indispensable conditions of scientific data are intelligent simply because they are also the conditions of life or survival. We follow them in every activity that entails vital welfare. We could not go on living unless we followed the three postulates of objectivity, measurability and causality of our environment. A man crossing the street in traffic would not last long if he ignored the objectivity of the machines and the street, the measurability of the distance of the machine closest to him and the sequence of events or the relationship of the machines to himself.

Science is simply a refinement of these three conditions of life, in that its observations are more keen and detailed, its measurements more minute and its laws of causality therefore more dependable. It is because science follows these common-sense postulates of life that it has been able to contribute most to the promotion of life.

The third principle is one of caution prompted by the second principle, namely, that since scientific knowledge is based upon reliable evidences the scientist, as scientist, must keep within his evidences in all his conclusions and, furthermore, that no matter how reliable his evidences his conclusions are nevertheless only tentative, since further evidences might call for modifications. Consequently, when a scientist makes unqualified statements he ceases being a scientist and becomes a dogmatist. This is unintelligent because dogmatism obstructs progressive knowledge. Therefore, this third principle of skepticism even in the face of reliable evidences is intelligent because it is a safeguard against the delusion of the absolute. It is not the evidences that are in doubt, but the conclusions, because they may be based upon insufficient data.

The fourth principle follows from the others, namely, that since science is reliable knowledge it does not call for defenses, arguments, justification, denials or affirmations. It stands on its own feet. The scientist does not argue, plead, exhort, split logical hairs; he investigates and reports his findings as far as he has gone and for whatever they may be worth. When he begins to affirm, deny, defend or justify he is no longer a scientist, which means he is no longer intelligent, for there is no intelligence where there is no examination and investigation.

This, then, is science. It is knowledge that is reliable, dependable, because it is knowledge that is derived from reliable sources, knowledge that we can believe, knowledge that does not call for indoctrination or justification and knowledge that is constantly growing in accuracy and dependability. It is knowledge that grows out of life and therefore contributes to life.

If this account of science is accurate, then we must conclude that the philosophical and religious deductions from physics are unscientific, and the men who make them are not scientists, since in their inferences they violate every one of the principles of science. They draw deductions beyond and outside the intention of the evidences, they use that which they can believe to justify and defend that which they like to believe, and they argue and dogmatize.

Let us now turn to philosophy. What is philosophy and who is the philosopher? In a measure all human beings have a philosophy and all are philosophers. Only some are foolish while others are wise, depending on where their philosophy comes from. Philosophy arises from the insufficiency of science alone to satisfy life. The statement that man lives not by bread alone is a digest of the story of philosophy. The bread of life is science, knowledge. It sustains life, nourishes it and keeps it well. Life could not remain life without knowledge, and the more knowledge there is the safer and more abundant life becomes. But life to a human being is more than the sustaining of life. It includes also the meaning of life. There are, for human life, the actualities that must be observed if life is to continue and increase in safety, but there are also the ideals that call for realization if the life is to be a truly human life. For human beings, life as *is* assumes human significance only in the light of life as it can be. What can life be as seen from what it is, what are its potentialities, what is the life that human beings can live, that is worthy of them to live as

human beings—all these are questions that human life raises and call for an answer. These questions go beyond the bread of life, and science has no answer for them and does not pretend to deal with them. But any answer to these questions must be based upon science if it is to have any validity, since it is only science that supplies valid data about the world and life. Now this is philosophy. Where science seeks truth about the world and life or facts and guards us against fancies, philosophy seeks truths for life or values to guard us against delusions. Philosophy is interested in the truths of science for the promotion of the truths of life. So where science analyzes philosophy synthesizes. Life must be seen not only as parts, but also as wholes, for the part is meaningless without the whole of which it is a part. The outlook of philosophy is therefore universal, while that of science is particular. Science sees the whole from the parts, philosophy views the parts from the whole. And both are indispensable, since a false view of the parts will lead to a delusion of the whole, and *vice versa*. This is the relationship of science to philosophy. The philosopher must have science if his philosophy is to be sound, and science calls for philosophy if its fruits are to be used in the interest of life. Progress in philosophy is dependent upon progress in science, but the progressive use of science in the service of life is dependent upon philosophy. But, and this is the crucial point, the philosopher, though he builds upon science and strives to be scientific in his thinking, does not claim scientific status for his philosophy. It is *his* philosophy, *his* cosmic, universal home, built out of the raw materials of science, and valid only for those who find his home comfortable. There is something compulsory in science—a man had better follow it; but there is nothing compulsory in a philosophy—a man might follow it. If a philosopher proclaims his philosophy as the only valid outlook he is no longer a philosopher, but a dogmatist. As a philosopher he distinguishes between what he likes to believe, namely, that his philosophy is universal, and what he can believe, namely, that it is his philosophy, having universal validity for him alone. In this respect he is in keeping with the spirit of science, although he is not a scientist.

From this analysis of the nature of philosophy we must therefore draw the further conclusion that the scientists here under discussion are no more philosophers in their philosophizings than they are scientists in the inferences they draw from physics. They do not build a philosophy out of science, but use physics to bolster up a preconceived philosophy for which they claim universal validity on the grounds that it is scientific. They are as anti-philosophical as they are anti-scientific.

Now as to religion. There are two views of religion: religion as belief in God, and religion as experience of God. The first is organized, established or public religion; the second is personal or private religion. The difference between the two is of vital importance because of their social consequences. The religious belief in God posits an objective deity whose existence is to be demonstrated by various proofs and belief in whom is to be indoctrinated or inculcated in various ways. This deity calls upon some persons to be his intermediaries and interpreters to man, and thus gives rise to a priestly class whose divine duty it becomes to instruct mankind in the will of this deity and whose divine privilege it is to intercede for man to this deity. This view of religion inevitably gives rise to a variety of religions, each holding a certain view of the nature of the deity, his will, his relation to man, and each proclaiming itself directly or indirectly

as being the one and only true religion. The social consequence is division among men, enmities, suspicions, propaganda, indoctrinations and intolerance.

Religion as experience of God is the religion of creative experience, with God the creative spark in man urging him on to grow in creativeness, in manhood. In this religion there is no room for sin other than ignorance of one's real self and no room for virtue other than knowledge of this self. So God becomes the highest self of mankind in whom man is to find his real welfare. Salvation for man is no easy attainment by accepting certain beliefs and engaging in certain mysterious practices, but is to be a constant seeking within oneself for that spark of divinity, which, as it is being found transforms its finder into itself. For this religion God has no being apart from man, and man has no being apart from God. As man is growing in manhood he is growing in Godhood, and thereby discovering God.

Now, the only relation between science and these two views of religion is this: that a scientific approach to religion shows that from the standpoint of human nature the religion of belief in God is a distortion of religion because it is a distortion of man, spreading confusion and enmity among men, while the religion of experience of God is true to human nature, for if it were in operation it would spread peace on earth and good-will among men. And it is this distorted, unscientific view of religion that current physics is alleged to support. Obviously an unscientific idea of religion can be supported only by an unscientific or distorted view of science, so that our theological-minded scientists violate science in support of a theological conception of religion. An objective God is not a religious experience but a theological dogma, and when science begins to support dogma it ceases being science, for science and dogma can not keep house together. Those scientists, then, who argue from physics to God and religion, use physics in support of an unscientific God, do so in an unscientific manner, and therefore only succeed in being anti-religion, although claiming to defend it and support it.

The conclusion is then that these men, speaking as scientists for philosophy and religion, are neither scientists nor philosophers nor religious thinkers. They are only theologians with a vengeance, for the theologian does not claim to speak as a scientist, but uses science as a prop whenever he can do so conveniently, while our scientists talk theology, and poor theology at that, as scientists in the name of science, to produce the impression that their theology is science. The result is that whereas the theologian of old was an open menace to science by trying to dictate to the scientist what he should or should not find in his investigations, the theological-minded scientist of to-day is a lurking menace in that he poses as a scientist in his theology and gives the theologian alleged scientific grounds for returning to his old practice of meddling in science.

ZERO AND THE CALENDAR

By Dr. R. M. WINGER

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OUR western (new style) calendar has been in use in parts of Europe since the time of its promulgation by Pope Gregory (1582) and in England since 1752. In America there is scarcely a household that is not supplied with at least a dozen every year. Yet ignorance of the calendar is at once profound and universal. The following generalizations seem warranted by the writer's experience:

A majority of the college population (including members of the faculty) are aware that leap year comes at intervals of four years.[1] But a greatly reduced number can state the rule governing the exceptions which excludes such years as 1900. A still smaller number know how the date for Easter is fixed. Fewer yet perhaps will answer with certainty when asked whether the death of Washington (1799) occurred in the last year of the century. And those who can compute the age of the emperor Augustus (63 B.C.–14 A.D.) are as scarce as those who understand Einstein.

The year just closed revived the question of the proper method of reckoning the time between two events, one B.C. and one A.D. For the classical teachers and scholars from Ireland to Australia selected 1935 as the year in which to celebrate the bimillennium of the poet Horace, born 65 B.C. Likewise the celebration of the bimillennium of Vergil, born 70 B.C., was initiated by the Italian Government with elaborate ceremonies and the issue of memorial stamps in 1930. And it is reported that plans are under way in Rome for the observance of the bimillennium of Augustus in 1937.

What is wrong with these dates? The trouble is that a bimillennium represents a lapse of 2,000 years, whereas the time between each pair of dates in question is 1999. The error arises in computing time as if 65 B.C., for example, were –65 years and 1935 A.D. were +1935. Numerous familiar scales of measurement are indeed constructed on this principle. Thus north and south latitude may be regarded as + and –, respectively. So also may west and east longitude, as well as temperature above and below zero. In each of these systems, however, as in the algebraic number scale, there is a number zero which divides the positive numbers from the negative, itself belonging to neither class. Thus the latitude of all points on the equator is zero, as is the longitude of all points on the prime meridian. *But there is no year zero in the (historian's) calendar.* Or, as Kubitschek[2] says, "The years 1 B.C. and 1 A.D. of ordinary usage follow one another directly without the intervention of zero." (See Diagram.)[3] Hence it is clear that

[1] The Roman priests were not even clear on this point. Thus during the first 36 years after the reform of the calendar by Julius Caesar, the priests decreed a leap year every three years. The resulting error of three days was corrected by Augustus, who omitted leap years altogether for a period of 12 years, restoring the Julian calendar.

[2] *Grundriss der Antiken Zeitrechnung*, München (1928), p. 13. See also Encyclopaedia Britannica, 11th ed., article "Chronology"; Giry, Manuel de Diplomatique, Paris, (1894), p. 89, footnote 2: "After the mode of reckoning in use, the year 753 of Rome is the year 1 B.C., the year 754, the year 1 A.D. Between the two periods ascending and descending, the computers have omitted zero"; Smith, *American Mathematical Monthly*, 38 (1930), p. 371; Herschel, "Outlines of Astronomy," 11th ed., p. 673.

[3] The Christian era thus dates from January 1, A.D. 1, while the birth of Christ is placed officially on December 25, of the year 1 B.C.! Authorities differ concerning the actual date of the birth of Christ.

a child born on December 8, 1 B.C. would be one year old (and not two) on December 8, 1 A.D. By the same token if Horace was born on December 8, 65 B.C., he would have been 65 years old on December 8, 1 A.D. (had he lived) and his bimillennium would occur 1935 years later, *i.e.*, on December 8, 1936.[3a]

Generally we have the rule for computing the time between events B.C. and A.D., as stated by Herschel (l.c.): "The sum of the nominal years B.C. and A.D. must be diminished by 1." This rule is emphasized by all the authorities on chronology from Ideler[4] (1825–6) to Kubitschek (1928), including the writer of the article "Chronology" in the Encyclopaedia Britannica, 11th edition, though I fail to find any mention of it in the latest edition of the Britannica either under chronology or calendar. Indeed two scholars, one of them a historian, writing for this work, have erred on this very point. In the article on Augustus occurs the statement: "He died on Aug. 19, A.D. 14 . . . on the anniversary of his entrance upon his first consulship 57 years before (43 B.C.)." Again in the article on Ovid, it is stated that the poet was born on March 20, 43 B.C. and that "He died in his sixty first year in A.D. 17." Both of these writers have plainly neglected to diminish by 1 the sum of the nominal years B.C. and A.D.

If those who wish to honor the bimillennial birthdays of ancient poets choose to ignore this rule and count inclusively, as the Romans did, that is their own affair.[5] Even they perhaps might demur at a hotel bill computed on this principle —for to the Romans "three days ago" meant day before yesterday. This inclusive counting, by which "every fourth year" was interpreted to mean once in three years, almost wrecked Caesar's calendar reform.[6]

It should also be observed that the number denoting the year in the calendar must be interpreted in the ordinal sense. The present year, *e.g.*, is the 1936th of the Christian era, but 1936 years will not have elapsed until the end of December 31.[7] The beginning of the twentieth century, therefore, about which there was much confusion and great argument, began with the beginning of the year 1901.[8]

Since there is no available event to mark the beginning of historical time, it was a stroke of genius on the part of Dionysius Exiguus (Denys the Small) to propose that the years be numbered in both directions from some intermediate event, such as the birth of Christ.[9] Most of the confusion mentioned above would have been avoided had he inserted a year zero in its proper sequence in the calendar scale. But historically there is the best of reasons for its omission: The number zero was probably unknown in Europe at the time the Dionysian system was adopted, in the sixth century. Zero seems to have been originated by the Hindus and used as a part of their positional system of notation. In this connection its function is to fill the empty places in such numbers as 4004 and it

[3a] *Cf.* Pogo, *Popular Astronomy*, March, 1936.

[4] "Handbuch der Mathematischen und Technischen Chronologie," two volumes.

[5] Fotheringham, writing for the *Classical Review* (Vol. 34, 1930), recognizes the absence of a year zero and argues for 1931 as the proper year in which to celebrate the Vergil bimillennium.

[6] Footnote 1.

[7] This is well understood with respect to the days of the month. March 10 is the name of a day, the tenth of the month, but 10 days of March will not have passed until the midnight between March 10 and March 11.

[8] Even Eells, correcting his error in counting anniversaries (*School and Society*, December, 1930), says: "This is due to the fact that the first year of the Christian era was numbered one not zero while the first year of every century since begins with '00'." But surely 1 + 100 = 101, hence the second and every century thereafter begins with —01.

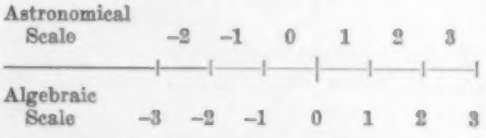
[9] He took for the beginning of the Christian era, March 25, 1 B.C., *i.e.*, the date of the Annunciation.

may have been a mere symbol of vacuity. Just when the concept of zero as a genuine number emerged is not clear, though rules for its combination with itself and other numbers in arithmetical operations are given by Brahmagupta (*c.* 628). [10] Recent research seems to confirm the view that a place-value zero was known in India as early as 500 A.D., or possibly earlier.[11] But the earliest occurrence in India of the symbol 0, according to D. E. Smith,[12] is in an inscription of 876; while the oldest European manuscript which contains the Hindu numerals (without the zero, however) was written in Spain in 976.

Long ago Ideler (*loc. cit.*) remarked on another difficulty of the historian's calendar, namely, that leap years are now numbered, 4, 8, 12, etc., A.D. but 1, 5, 9, etc. in the era B.C. And he observed that if 1 B.C. were changed to 0, 2 B.C. to –1, etc. not only would leap years be named by multiples of 4 in all cases, but the time between two events before and after the beginning of the Christian era would then be found correctly by adding the number of positive and negative years. This method of reckoning time, now generally adopted by astronomers, was introduced by Jacob Cassini, according to Kubitschek, and first used in the introduction to his "Tables Astronomiques," 1740, page 5. Schram,[13] commenting on this system, says:

> Zero is often falsely understood and identified with "nothing," whereas it is nothing but the number coming before unity in the natural number scale. No one will deny that 18, 19, 20, 21, 22 are five consecutive numbers which might denote any five consecutive years; then an interval ten years earlier would be denoted by 8, 9, 10, 11, 12, and an interval ten years still earlier by –2, –1, 0, 1, 2. This however the historians will not recognize but call these years 3 B.C., 2 B.C., 1 B.C., 1 A.D., 2 A.D., so that zero has been thrust out of its natural number sequence. Thus we have two systems of numbering years before the birth of Christ, the astronomical and the historical which differ from one another by one unit.[14]

In the astronomer's calendar, the birth of Augustus would be placed in the year –62, consequently his bimillennium, computed as in elementary algebra, would fall in 1938. There is one important difference, however, between the astronomer's calendar and the algebraic scale: When plotting numbers in algebra, each number corresponds to a single point, 0 marking the point of origin. In the calendar, on the other hand, each integral number designates an entire year. (See accompanying diagram).



In the algebraic scale we have naturally made the number 0 correspond to the beginning of the Christian era. On the algebraic scale the number attached to any point, whether integral or not, indicates the exact time interval measured from the beginning of the Christian era. Thus April 1 of the year 3 (A.D.) corresponds on the algebraic scale approximately to 2.25, which is the time elapsed from the beginning of the Christian era. Again April 1 of the year –2 would be represented on the algebraic scale approximately by –2.75, which gives the number of years before the era.

The astronomer's calendar would more nearly conform to our experience in measuring if it had placed the beginning of the Christian era at the beginning of the year 0 instead of at the end. This could be accomplished by sliding the whole astronomical scale of the above diagram

[10] Datta, *Bulletin, Calcutta Mathematical Society*, 18: 165–176, 1927.

[11] See a series of papers with references in the *American Mathematical Monthly*: (1) by Datta, 33: 220, 449, 1926, also 38: 566, 1931; (2) by Ganguli, 34: 409, 1927; 39: 251 and 389, 1932; 40: 25 and 154, 1933. Or, Datta and Singh, "History of Hindu Mathematics," Lahore. 1935.

[12] "History of Mathematics," Vol. II, p. 69 and 75.

[13] "Kalendariographische und Chronologische Tafeln" (Leipzig, 1908).

[14] Quoted from Kubitschek, *loc. cit.*, p. 13.

one unit to the right, making 0 the first year of the Christian era. The dates B.C. would then be the same as in the present historian's calendar, while each date A.D. would be increased by 1.[15] The number denoting the year A.D. would then indicate the integral number of years elapsed from the beginning of the era. Now April 1, A.D. 2 (or the year +2) would correspond on the algebraic scale to 2.25, and the time elapsed is 2 + .25 (the time from January 1 to April 1); and April 1, B.C. 2 (*i.e.*, the year −2) would correspond on the algebraic scale to −1.75, which is −2 + .25 (the time from January 1 to April 1). Thus in each case the fractional year added is positive—due to the fact that time moves forward—while the integral year, *i.e.*, its ordinal number in the calendar, may be either positive or negative. This is entirely analogous to the notation used in writing logarithms, where commonly the mantissa (the decimal part) is taken as positive, whereas the characteristic (the integral part) may be either positive or negative.

It is unfortunate that a zero year has not been introduced in the calendar of the historian and the chronologist. And since zero in the calendar stands for a whole year and not a single instant of time as it would in the algebraic scale, perhaps the astronomer's choice is the most satisfactory under all the circumstances. That would require only the change of the dates B.C., relatively few of which are known with certainty. The years of the Christian era would be numbered as at present, starting with 1, as we number the days of the month, automobile licenses, the pages of a book.[16] The chronologists could institute such a reform by simple agreement among themselves. If they are unwilling to insert a zero year in the calendar, then let the writers of school histories explain its absence and the embarrassment which its omission entails. In the matter of reckoning time, "It wad frae mony a blunder free us."

Many of the difficulties of a satsifactory calendar are inherent. Foremost of these perhaps is the fact that the three most natural units of time, namely, the periods of the rotation of the earth on its axis, the revolution of the moon about the earth and the earth about the sun are incommensurable. That is, neither the year nor the (lunar) month contains an integral number of days, nor is the fractional residue of a day a rational number such as $\frac{1}{4}$ or $\frac{1}{3}$. Some of the anomalies of our present calendar are accidental or at least avoidable, such, for example, as the incidence of New Year on different days of the week and of Easter on different days of the year in two different months. The correction of these irregularities was the hope of a vigorous agitation for calendar reform a few years ago, which however has somewhat subsided of late.

There remain serious problems of chronology which have been bequeathed to us from the past. A major source of confusion has been the use of different calendars by different countries. Another is the use of different calendars by the same country at different periods of its history or even the same period. For example, the civil year and the ecclesiastical year have not always agreed, while the astronomical year agrees with neither. Indeed, there are several astronomical years all in good standing.

Not only has the length of the year varied, but the beginning of the year has been fixed at numerous dates.[17] Among the dates selected to mark the beginning of the year, four are related to the life of Christ: March 25 (style of the annunciation), December 25 (style of the nativity), January 1 (style of the circumcision) and Easter (style of France).

[15] Leap years would then be numbered 3, 7, 11, etc., A.D., B.C. dates being unaffected.

[16] The whimsical author William DeMorgan, I believe, called the first chapter of one of his books Chapter 0; and I have seen a set of mathematical axioms numbered, 0, 1, 2, etc.

[17] See particularly Giry, Chapter II.

Others were doubtless determined by the seasons. Both the vernal and autumnal equinoxes have been used.[18] Also may be mentioned September 1 and 24 (Greek), March 1 (Roman religious and German) and August 11 (Danish). Likewise the beginning of the day has been reckoned variously from sunrise, noon, sunset and midnight. Again every instant of day and night on a fixed meridian is (solar) noon on some other. And in crossing the international date line one changes time by an entire day.

With all this multiplicity of calendars, whose months and years are of varying length, together with irregular and sometimes capricious intercalations of days and months, it requires no imagination to perceive that the problem of an accurate historical chronology is one of the greatest complexity. To quote Herschel, page 674:

> The history of the calendar, with reference to Chronology, . . . may be compared to that of a clock, going regularly when left to itself, but sometimes forgotten to be wound up; and when wound, sometimes set forward, sometimes backward, either to serve particular purposes and private interests, or to rectify blunders in setting.

And he might have added that the clock records no zero time!

THE ANTIQUITY OF MAN IN AMERICA

By Dr. E. B. HOWARD

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THAT man may have lived in North America for a good many thousand years has been a subject for discussion off and on since the middle of the eighteenth century; but these discussions attracted very little attention till discoveries in Europe demonstrated the existence of man at an early period on that continent. These disclosures tended to promote search in America for evidence of an equally early culture. The period immediately preceding and subsequent to the Civil War was marked by such announcements of the finding of traces of early man as those at Trenton, N. J., those from the auriferous gravels of California and others from Minnesota, Kansas, and so on.

EARLY APPEARANCE NOW CONCEDED

One by one these claims to antiquity were attacked by scientists who could not subscribe to the theory that man had existed in the New World for such a long time. In spite of this, however, reports of discoveries of extinct animal bones in association with human remains or objects of human manufacture continued to appear from time to time in North America, until finally the evidence from Folsom, New Mexico, made it apparent to every one that man was present in that region at an earlier period than many supposed. This discovery immediately galvanized general interest in the subject of man's antiquity and promoted activity along this line in various parts of the country.

In the light of these results and others subsequently obtained, I doubt if there is any one to-day who is not willing to concede that the evidence is ample for believing that man had been in North America for several thousand years before the beginning of the Christian era. This, it will be admitted, is not very satisfactory dating, but it will have to suffice till more is known about glacial

[18] Christmas and January 1 too come near the winter solstice and Easter near the vernal equinox. January 1 marked the beginning of the Roman year 150 years before the Christian era.

geology, and more particularly with regard to those regions that were not glaciated during the Wisconsin stage.

What is becoming increasingly clear is that the associations occurring at Folsom, at Clovis and at other sites, more recently investigated, point to climatic conditions that were different from those that exist at the present time in the same regions. The evidence is for a colder and a more moist climate at the time the deposits were laid down, and in some localities the types of shells and microscopic examination of samples from the deposits indicate a progression towards a milder climate, with more arid conditions. Many of the animal bones represent types that are altogether extinct to-day in the region in which they are found, or represent types that to-day live in entirely different environments.

Evidence Obtained at Clovis

To be a little more specific, we may now consider the results obtained at Clovis, New Mexico. Here there is such evidence as we have been discussing that man lived in the region at a moderately early time. Taking it up briefly, we may consider the subject from the three points of view of geology, paleontology, and archeology.

The physiography of the Clovis region presents an almost flat surface, which is characteristic of the Staked Plains of eastern New Mexico and northwestern Texas—a surface that is almost uneroded. There are, however, a series of basin-like depressions, with surrounding sand-dunes, that break the monotony of the landscape between Portales and Clovis. These basins are made up of bluish-gray silt and clay deposits, probably representing old lake beds, which once formed part of the Brazos River system, when that river extended much farther west.

These deposits have been exposed by the wind, and it is in and on the bluish-gray clays that the bones of the bison and the elephant are found, and in which worked flints and lenses of charcoal appear. A casual examination of this area will lead one to form the opinion that not much can be done in the way of determining the age of these deposits, but a more thorough study reveals the fact that the evidence fits well into the general picture as it is being developed in the Southwest.

Dating the Beds

In an attempt to date these beds at Clovis, Dr. Antevs spent some time summer before last investigating the evidence in New Mexico of geological features that might tie in with the bone-bearing strata at Clovis. Based upon a study of the data obtained, near Willard, New Mexico, almost due west of Clovis in the center of the state, where well-developed shore lines show that an ancient lake had existed, Dr. Antevs believes that the highest stand of the lake, at that earlier time being about 150 feet deep, and covering about 450 square miles, corresponded to a period somewhat earlier than that when modern temperature conditions were attained in the Southwest.

Dr. Antevs accordingly places the high stand of this ancient Lake Estancia at about 15,000 years ago, and he estimates the time when the clays were finally laid down at Clovis at 12,000 to 13,000 years ago. Apparently the estimate of 10,000 years, previously made for the age of the Clovis discoveries, seems to have been conservative.

Clovis and Folsom Finds Compared

Of the animal bones discovered at Clovis, those of the mammoth and bison are found in the bluish-gray deposits, the horse and camel in the top of the underlying beds. Dr. Stock is at present occupied with the identification of the specimens from this site, and his report

is looked forward to with interest. It was the exposure of bison and mammoth bones in nearly every one of these basins that first suggested the possibility of association with human objects, since quantities of flint tools and chips were scattered over the surface of these same basins.

Without the knowledge gained by a study of the Folsom site that at Clovis would have presented a very much more difficult task to interpret, bearing in mind that the Lindenmeier discoveries, near Fort Collins, Colorado, had not yet been announced by Dr. Roberts. Everything about this newest discovery of Folsom artifacts in Colorado tends to show that the same people had lived at Clovis at about the same time. Identical types of artifacts are found at each place, and the geological evidence in each place seems to point to a time when climatic conditions were different than at the present time, as already mentioned.

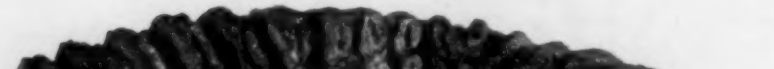
TYPES OF EARLY ARTIFACTS

THE FIRST IS A "FOLSOM-LIKE" POINT, SOMEWHAT CRUDER THAN THE SO-CALLED "FOLSOM" TYPE; THE SECOND IS A "YUMA" TYPE, SHOWING FINE, RIBBON-LIKE FLAKING ON ITS FACE; THE THIRD IS A KNIFE; THE FOURTH REPRESENTS THE "FISH-TAIL" TYPE OF THE "FOLSOM-LIKE" POINT.

THE FOLSOM TYPE OF SPEARPOINT

Though end- and side-scrapers were found at Clovis, the Folsom point is the important type that furnished the clue to the site in the first place. This particular type of spearpoint can be briefly described as a thin leaf-shaped point, the chief characteristic being a longitudinal groove along each, or sometimes only one face. The base is concave, with ear-like projections. The secondary chipping is very fine and shows remarkable control of the flaking tool.

The general character of these points, as well as those of the equally well-



TOOTH OF EXTINCT ELEPHANT

FROM THE CLOVIS, NEW MEXICO, GRAVEL-PIT. THE NUMBER OF BONES OF EXTINCT BISON AND ELEPHANTS FOUND IN THIS PIT IS SO GREAT AS TO SUGGEST THAT IT ONCE MAY HAVE BEEN A WATERHOLE IN WHICH THESE ANIMALS BOGGED DOWN.

chipped points known as Yuma points, has led to comparison of these early American artifacts with those of the European Solutrean culture. There is no doubt that there is a striking likeness in some of the shapes of both, and that both types represent pressure flaking of a high order. Nevertheless, after examining a number of Solutrean blades and points last summer in France, one feels the necessity of reserving opinion upon the subject till a more detailed study is made of the whole situation. Off hand it seems unlikely that there can be any direct connection between the two cultures, but there is a possibility that both may trace their beginnings to Asia.

As a matter of fact the Danish neolithic points and daggers offer more exact comparison with the points of our Folsom-Yuma complex than anything else in Europe, except, of course, that none of the Danish points have the longitudinal groove, characteristic of the Folsom point. This particular grooved type, so far as I have been able to ascertain, has not been observed anywhere in Europe or Africa.

Last summer, in order to find out whether anything approaching the Fol-

THE JAWBONE OF EXTINCT BISON

FOUND IN A GRAVEL PIT NEAR CLOVIS, NEW MEXICO. CHARCOAL AND BURNED BONES, EVIDENCE OF EARLY MAN, WERE ALSO FOUND HERE.

som point had been found in Siberia, I went to Russia under a grant from the American Philosophical Society, and at Leningrad had an opportunity to study the collections in the museums from Siberia. There were no types like our Folsom points, but, as archeological material of this nature from Siberia was limited, it may not be wise to make any sweeping statements as yet about these highly specialized artifacts or their prototypes from that country.

Folsom-like Points

The facts as observed point to the flaked groove point as being a development of North American origin. Dr. Hrdlička reports the finding of rubbed groove points of slate from Alaska, but these are of a later period than the Folsom points. A technique similar to that of these Alaska points has been reported from Manchuria, said to be of the pre-Han period.

To come back now to the Folsom point, it should be noted that its distribution is somewhat confused by the fact that there are grooved points found in other parts of North America as well as the Southwest, which differ from the typical Folsom point as found at Folsom and other relatively near-by sites. Whether they are earlier, later or contemporaneous with the true Folsom type can not be determined at this time.

We found one of these more generalized types of Folsom point called Folsom-like points, in a cave in the Guadalupe Mountains of New Mexico, southwest of Clovis, at a level definitely below that of a Basket Maker people who had used the cave for burial purposes. These lower levels of the cave contained numerous lenses of charcoal and the bones of a number of animals that have been extinct in that region for a long time, such as those of the camel, horse, antelope, bison of extinct species and musk-ox.

Not, however, till more facts are gathered upon the subject of glacial geology and the question of post-glacial climatic fluctuations, and until more is known of the types of stone tools made by our prehistoric peoples, shall we be able to project upon the screen of research a clear picture of what America was like before the Christian era.

THE REAL WINNERS IN THE 1936 OLYMPIC GAMES

By Dr. CHARLES D. SNYDER
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PERHAPS it has occurred to others besides myself that the absolute scores accredited to the various countries in the Olympiad just ended in Berlin somehow do not represent their respective relative positions of excellence.

For one thing the number of people in each of the countries from which their representatives were chosen is a vastly various quantity. It is not unreasonable to assume that, given equal opportunity and training, the human animal ought to wrest his share of honors, in such a variety of sports as the Games included, regardless of the country he represents. This being granted as an ideal state of affairs, it would follow then that the total score of points won by any one country may well be in direct proportion to its population. If in reality this turns out not to be the case, then may we begin to search for the causes of the differences.

In order to make use of the population factor in revaluating the scores, I have made use of "the unofficial" final scores accredited to each nation in the Associated Press report as published on August 17, 1936. The dispatch listed 28 countries. Of these I omit the three countries which, in any way one treats the figures, obviously will stand at the bottom of the list, namely India, Roumania and Turkey. From standard references I have noted down the population figures for all the rest of the 25 countries listed, together with the "unofficial" score assigned to each. Adding up the two columns gives a grand total population of 560.83 millions in all the countries considered, whose total scores likewise sum up to 533 points. If one now divide the total number of points by the total population expressed as millions, one has a fraction of 0.986, which represents the distribution of points for each million of the total of all populations represented in the Games.

To get the ideal score for each country then, all one need do is to multiply the population of each country by the factor given above, namely, 0.986. This ideal score then may be compared with the score actually won, and the question as to how nearly each country by its earned score has realized its ideal score may be answered in terms of per cent. This is done by dividing the earned score by the ideal score and multiplying by 100.

The results of such an inquiry, together with the pertinent data, appear in Table 1, where the first column of figures gives the population in millions; the second column, the respective "ideal score" cal-

TABLE 1
THE 1936 OLYMPIAD. ALL EVENTS REVALUATED ON A POPULATION BASIS

Country	Population in millions	Ideal score, on population basis	Actual score earned	Percentage of ideal score realized
Esthonia	1.17	1.15	6	522
Hungary	8.67	8.53	43	517
Sweden	6.10	6.01	25.5	508
Finland	3.63	3.57	17	476
Austria	6.68	6.58	26	396
Norway	2.65	2.61	9	345
Holland	7.83	7.72	24	311
Switzerland	3.88	3.82	10.5	275
Germany	64.60	63.60	134	195
Argentina	10.92	10.77	16	149
Canada	9.66	9.52	9	94.5
Italy	41.49	40.80	55.5	87.0
France	40.92	40.30	31	86.9
U. S. A.	121.74	119.80	77.5	64.8
Belgium	8.05	7.94	5	63.1
Czechoslovakia	14.73	14.53	9	62.0
Denmark	3.48	3.43	2	58.4
Uruguay	1.81	1.78	1	56.3
Mexico	14.80	14.60	8	54.8
Egypt	14.18	13.97	7.5	53.8
Great Britain	43.03	42.40	20.5	48.4
Poland	27.18	26.90	11.0	40.9
Yugoslavia	13.42	13.23	2	17.65
Philippines	11.57	11.4	2	17.5
Japan	76.69	75.5	11	14.6
Totals	560.83		553.0	

culated on the population basis, as indicated above; the third column, the earned score as accredited by the Associated Press dispatch. The last column contains the percentage of its ideal score that each country has realized by its earned score. It is this last figure which determines now the real position of excellence of each country in respect to all the others. Indeed, in the final construction of the table, and for the convenience of the reader, the order of naming the countries (and giving their appropriate data) was selected on this basis. It is needless to remark that order of excellence here turns out to be quite different from the order when based on the absolute number of points won.

Inspection of the table reveals some startling facts. Germany and U. S. A. no longer lead, but appear ninth and fourteenth; Japan drops down from twelfth place to the end of the column. It becomes quite obvious that, under present conditions at least, it is the snug little countries of northern Europe that can win from 2.5 to 5 times their alloted ideal scores, whereas the countries of larger populations, wherever they are, can only take middle positions, realizing at best only from ¾ to 2 times their allotments on a population basis. The U. S. A. with her 64.8 per cent. can only boast of leading those countries which have definitely failed to hold their own, including Great Britain, who has failed badly.

Many of us still consider the events of track and field as the noblest sports, and since they represent more nearly the games played in the original Olympiads it is of interest to revaluate the scores accredited to the various countries for these events alone, as we have done above for all events. This has been done using the unofficial scores again (for both men and women) as they were compiled in the Associated Press dispatch of August 9. The results appear in Table 2. And it is important to compare this table for track and field with that of all events.

TABLE 2

TRACK AND FIELD EVENTS REVALUATED ON A POPULATION BASIS*

Country	Population in millions	Ideal score	Actual points won	Percentage score realized
Finland	3.63	4.54	80 1/4	1776
New Zealand .	1.54	1.9	10	579
Sweden	6.10	7.6	19 1/11	251
Canada	9.66	12.1	30 1/11	248
Switzerland .	3.88	4.7	9	193
Holland	7.83	9.8	16 1/3	167
Norway	2.65	3.3	5	153
Germany	64.60	80.1	121 1/4	150
U. S. A.	121.74	152.2	225 1/3	148
Latvia	2.35	2.9	4	138
Hungary	8.67	10.8	11 2/11	103
Great Britain.	43.03	53.9	53 1/11	100
Italy	41.49	51.9	36 13/22	71
Japan	76.69	96.1	58 13/22	61
Australia	8.18	10.2	4 1/3	40
Austria	6.68	8.5	3 2/11	38
Argentina ...	10.92	13.7	4	29
Philippines ..	11.57	14.5	4	28
Greece	6.20	7.9	2	25
Czechoslovakia	14.73	18.4	3 1/11	16 4/5
Poland	27.18	34.3	5 1/11	15
France	40.92	51.2	2 1/2	4 9/10
Brazil	39.69	49.7	2	4
South African Union	7.78	9.7	2/11	2

* Calculated from the Associated Press accredited scores of August 9.

In the first place, taking the countries all in all, the main lines of demarcation remain the same in both tables. The little countries of northern Europe still head the list, with little Finland doing nearly 18 times more than her share of realizing on her population basis. Germany and the U. S. A. realize nearly one and one half their shares, and Great Britain registers more nearly as we think of her, that is, as a country who takes her sports leisurely and with pleasure rather than grimly and strenuously. She scores just 100 per cent. of the points allotted her on a population basis. While Denmark and Belgium do not score enough to appear at all in the list, there are Latvia and Hungary just ahead of Great Britain. The lower half of the table is made up of those countries which realized in track and field less than 100 per cent. of the scores allotted them on the population bases. It includes now Austria and Argentina, each of whom for all-events realized more than 100 per cent. On the whole, however, for track and field, as for all events, the larger countries seem to be able to do no better than to take

middle positions, some of them again taking positions down at the bottom of the list.

One is tempted sorely to speculate as to the reasons for this great diversity in athletic prowess. When Owens was winning his wonderful races a great cry went up in the American press proclaiming that once and for all the myth of superiority of one race over another had been smashed. Perhaps in the short race (as has been suggested) some anatomical advantage of bone or muscle structure gives the black man advantage over the white. In any case we must remember that the Negro boys were trained by white men in the white man's institutions. Until some actual research is carried out we are obliged to regard slight anatomical variations among peoples not so much the determining factors for winners as are the more subtle qualities of character and spirit added to the well-trained body. Luckily the games run off at the Olympiad were varied enough to give all peoples some chance to show what they may do well and thus gain points.

It is generally stated by anthropologists that all the nations of the world are made up of mixed races. Some of them, however, are much more mixed than others. It surely is an arresting fact that the countries standing in the upper half of the table represent the homelands of what remnants there are of the once "great northern races," and that, with the exception of Denmark, those countries appearing in the lower half of the list are on the whole those whose populations represent the greatest racial mixtures.[1]

Looking again for a moment at the table for track and field one notes that it is just the little countries in the leading places that have no international debts, save Finland; and she, of all the European countries that have such debts, is the only one who has been meeting her obligations as they come due, a feat compared to which in these decadent days the winning of athletic games seems indeed to be mere child's play.

Political unrest and economic instability therefore may be playing a rôle, but just what is not clear. Since the late war Esthonia, Hungary and Austria surely have had their share of troubles, yet they score high in the all-events table; whereas comparative calm and security apparently have not helped Denmark and the Philippines to lift themselves out of the class of mediocrity.

By far the clearest point revealed by the tables is the fact that mere bigness does not guarantee superiority in athletic sports for any land. Rather it would seem that the results of the Games bear out what is known to be a truth, namely, that with spread of empire the quality of the average individual weakens, which in turn is probably largely due to increased racial interbreeding.

The world being what it is, it would be perhaps too much to hope that with the renewal of the Olympiads the nations of the world would so order their affairs that at some not too distant date the scores their youths bring home will approach more and more nearly 100 per cent. of their shares allotted on the population basis.

To hasten this day the committee in charge of the Games has done much, and may do more. First, by fostering to the utmost the friendly, generous spirit of sportsmanship; second, by adding to the number of events, to be held in the matches as many as possible of the games each country finds itself able to play well. Surely nothing the nations have undertaken together thus far has done so much for international understanding and appreciation as have the Olympic Games of the present century.

[1] The reader doubtless recalls arguments in favor of racial interbreeding. But athletic scores put on a population basis, as above, give no support whatever to such arguments, which after all are in the nature of wish fulfilments. The main thesis also of such articles as the recent one by Powers and Webster on "The Coloured Shadow in Sport" (*Liberty*, New York, and condensed in *Magazine Digest* for August, 1936) are thereby shown to be equally groundless.

THE PROGRESS OF SCIENCE

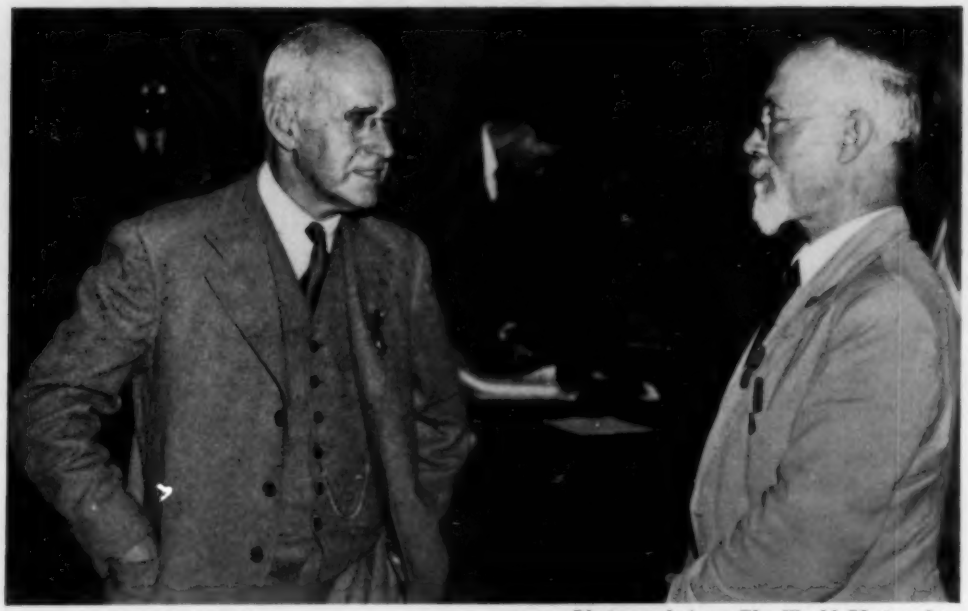
THE HARVARD TERCENTENARY MEETING OF THE AMERICAN ASTRONOMICAL SOCIETY

THE fall meeting of the American Astronomical Society was held at Harvard University during the first week of the Tercentenary Conference in September; sixty-six papers were presented at the morning sessions. The initial paper gave one of the outstanding contributions of the meeting. Professor E. W. Brown, of Yale University, reported on investigations dealing with the stellar case of the problem of three bodies. He studied the perturbations from the purely elliptical motion that will arise when the orbits in a close binary system are affected by the action of a distant third body. The problem reminds one of the lunar problem, but the essential difference lies in the fact that the mass of the sun is roughly 300,000 times the combined mass of the earth-moon system, whereas the three stars involved in the stellar case may be assumed to have comparable masses. Brown has found that the stellar case can be solved to a high degree of approximation if he includes only a comparatively small number of terms in his series for the perturbations of the elements and that his solutions will hold for practically any value of the eccentricity or inclination.

Drs. P. van de Kamp, A. N. Vyssotsky and Emma Williams-Vyssotsky, from the McCormick Observatory of the University of Virginia, reported on the analysis of their large material of proper motions for faint stars. Important information on the decrease of star density perpendicular to the galactic plane has been obtained and the rapid falling off found for the red dwarf stars was one of their most startling results. The discovery of faint stars of low temperature was the subject of two papers, one by Dr. C. W. Hetzler, of the Yerkes Observatory, and a second one by Dr. V. M. Slipher and Miss Alice M. Rogers, of the Lowell Observatory. Both papers gave lists of very red stars that have already been found, and Hetzler suggested a surface temperature of hardly more than 1,000° C. for some of the objects in his survey.

Nova Herculis was given a good deal of attention and reports on the changes in its spectrum were made by representatives from the Perkins Observatory, the Observatory of the University of Michigan and the Dominion Astrophysical Observatory. Velocities as high as 3,500 km/sec had been recorded for some of the expanding shells shortly before the disappearance of the continuous spectrum of the nova. Of particular interest was an attempt to determine the distance of the nova from the intensities of the interstellar lines by Dr. J. A. Pearce, which yielded an approximate value for this distance of 2,000 light years.

Drs. P. W. Merrill and O. C. Wilson, of the Mount Wilson Observatory, have made further studies of the intensities of interstellar lines. They find that the interstellar lines of unknown origin, towards the red end of the spectrum, are extremely strong and that their intensities exceed considerably those of the well-known interstellar D lines, owing to sodium, and the H and K lines, owing to ionized calcium. Dr. Orren Mohler, of the Cook Observatory of Wynnewood, Pa., reported on studies of solar radiation in the wave-length interval 2,000 to 2,300 Ångströms. He showed that some of the solar radiation in the ultra-violet will undoubtedly penetrate through our atmosphere in sufficient amount to be detectable at an altitude of only forty meters above sea-level. His observations did not indicate that the intensity of the radiation between 2,000 and 2,300 Ångströms in the true energy spectrum of



Photograph from The World Photos, Inc.

SIR ARTHUR EDDINGTON AND PROFESSOR ANTONIE PANNEKOEK

the sun exceeds considerably the amount to be expected for a black body at a surface temperature of 6,000° K.

The Harvard Observatory had its "birthday party" on Friday morning, on which occasion twenty-eight papers by members of the staff were presented. Miss Annie J. Cannon, who has done more work in the classification of stellar spectra than any one else in the world, told about the innovation of the Henry Draper charts of stellar spectra. Dr. Harlow Shapley and Miss C. D. Boyd presented results of a study that revealed for the first time variable stars well beyond the galactic center. Miss H. H. Swope reported on the discovery of a most remarkable variable star that had apparently changed from an irregular variable into a respectable Cepheid variable with a period that has gradually increased from 14 days in 1928 to 21 days in 1936. Dr. F. L. Whipple and Mr. L. E. Cunningham discussed the orbit of the asteroid Apollo, better known in the astronomical vernacular as "Reinmuth's object." This asteroid was only observed for less than a month, and the elongated shape of its orbit would make its rediscovery highly important. The calculations showed, however, that the chances for rediscovery are exceedingly small. Professor J. S. Paraskevopoulos, superintendent of the southern station of the Harvard Observatory at Bloemfontein, Orange Free State, South Africa, presented some remarkable photographs of southern nebulae and clusters. Professors D. H. Menzel and J. C. Boyce reported preliminary results of the "Harvard-M.I.T." eclipse expedition to Ak Bulak (USSR), where they found the corona to be at least fifty times as bright as the full moon.

The conferences on Thursday and Friday afternoons were undoubtedly the high points of the meeting. Professor Antonie Pannekoek, of the University of Amsterdam, Holland, told of his work on the interpretation of the continuous spectra of the stars. His paper gave a most lucid exposition of the complex problem of stellar surface temperatures. Professor Henry Norris Russell, of

Princeton University, the president of the American Astronomical Society, gave a summary of his extensive work on the masses of the stars. His investigation confirmed in a remarkable fashion Eddington's pioneer work on the mass-luminosity relation, and he gave important observational evidence bearing upon the hydrogen content of the stars. Dr. Th. Dunham, of Mount Wilson Observatory, discussed his work on the temperatures and pressures of the stars, and one of our most distinguished visitors from abroad, Dr. Megh Nad Saha, of the University of Allahabad, told of the importance of studies of the ultra-violet radiation of the sun. The final paper of the Thursday afternoon session was presented by Dr. Otto Struve, director of the Yerkes and McDonald Observatories, who reported on a series of elegant investigations on reflection nebulae made by himself and his associates. Struve showed from studies of the similarity in color between stars and associated nebulae that Rayleigh scattering plays no rôle, but that sizable particles with diameters of the order of one thousandth of a millimeter are largely responsible for the appearance of the reflection nebulae.

The main events of the Friday afternoon conference on cosmogony were addresses by Professor Tullio Levi-Civita, of Rome, Italy, and Sir Arthur Eddington, of Cambridge, England. Unfortunately illness of his wife prevented Professor Albert Einstein from taking part in this discussion. Professor Levi-Civita had as his topic "Astronomical Consequences of the Relativistic Two-body Problem," and Sir Arthur Eddington spoke about "The Cosmical Constant and the Recession of the Nebulae." Additional short papers were presented by Professor Arthur Haas, of the University of Notre Dame, and Professor H. P. Robertson, of Princeton University.

DRS. SAHA AND SHAPLEY AT THE HARVARD COLLEGE OBSERVATORY

DR. MEGH NAD SAHA, OF THE UNIVERSITY OF ALLAHABAD, EDITOR OF "SCIENCE AND CULTURE," WHO REPORTED UPON HIS RECENT STUDIES OF THE ULTRA-VIOLET RADIATION OF THE SUN; DR. HARLOW SHAPLEY, DIRECTOR OF THE HARVARD COLLEGE OBSERVATORY, WAS HOST TO THE VISITING ASTRONOMERS.

Probably the most exciting event of the meeting took place after the society banquet on Friday evening, when Dr. H. D. Curtis, of the University of Michigan, showed motion pictures of the sun taken at the McMath-Hulbert Observatory at Lake Angelus, Michigan. I doubt very much whether any one present on that occasion would ever have dared to predict that eruptive prominences could change in such a remarkable fashion as that shown by these motion pictures. The evident turbulent motion and the "sucking back" of some of the prominences into the solar surface were a most exciting sight. The motion pictures shown by Dr. Curtis had been taken only quite recently and the astronomical world looks forward to hearing about the details revealed upon further study.

The Harvard meeting of the American Astronomical Society was rich in solid contributions, and I feel that it can be fairly said that there has been at no meeting in the past such an indication of abundant astronomical activity in the United States.

BART J. BOK
HARVARD COLLEGE OBSERVATORY

TWO PAPERS IN THE BIOLOGICAL SCIENCES OF THE HARVARD TERCENTENARY CONFERENCE

THE MODE OF OPERATION OF THE "ORGANIZER"

PROFESSOR HANS SPEMANN outlined experiments on the development of amphibian larvae, carried out during the past fifteen years by himself and his associates, which have given much insight into the problem of why a particular region in the early embryo develops into a particular organ in the embryo. When a fertilized egg-cell has only just begun to develop and the future embryo is represented by little more than a mass of cells surrounding a central cavity, there is no obvious sign of dominance of one part over another, but Professor Spemann showed that one particular region, the dorsal lip of the blastopore, determines the formation of the primary axis of the embryo. A tiny piece of tissue transplanted from this region into another part of the same or a different embryo will induce the formation of a second axis of development, and a double embryo will form. This organization of an entire embryo by a little piece of one young germ placed in another is the basis of the original concept of the "organizer." This idea and its experimental development won for Professor Spemann in 1935 the Nobel Prize in physiology and medicine.

Professor Spemann explained that the starting point of this work lay in earlier experiments of the same sort, involving the removal of specific regions of the germ and their replacement by material from another place. If in a very young embryo of the Triton, a newt, a piece of tissue which would normally become brain is removed and replaced by tissue which would normally form skin, the latter does not become skin but brain. The identity of the transplanted and transformed tissue during its development can be proved by taking it from another species containing more or less pigment than the tissues of the host. This shows that in these early stages of development a given bit of tissue may become either skin or brain. Next came the question as to the nature of the influence proceeding from the underlying structures, the so-called mesoderm, which directs the development of the transplant into brain in one case and skin in another. A long series of experiments showed that many substances from animal tissues of the most diverse origins, from tape-worm to man, and even chemically pure substances of known constitution, exhibited this capacity of inducing differentiation. But the discovery of these wide-spread inductive effects ex-

erted by practically every tissue in the growing embryo is only one step in the solution of the problem, for it does not explain why the particular organ develops in its proper place, but only that development of some sort is induced. The answer is to be sought in part in the combination of the various influences from all the neighboring tissues. There is not only one but a whole system of "field-effects," proceeding from various regions, whose interplay determines development.

Professor Spemann further explained that many important factors also reside in the transplanted tissue itself. The stage of development at which the transplant is made may be decisive. Apparently at one time a given bit of skin is "loaded" to the highest degree with the ability to produce, let us say, the lens of the eye. If an inductor acts upon it at this moment of highest readiness a lens will be formed, while at a somewhat earlier stage it would have brought forth an embryonic brain. The factors inherent in the transplanted tissue itself appear still more clearly when tissue is transplanted between distantly related species. A piece of skin from the head region of an early toad embryo placed in the corresponding position in a Triton embryo will develop the suckers and the horny jaws characteristic of the toad instead of the corresponding balancing organs and teeth of the Triton.

Evidently, concluded Professor Spemann, the releasing stimulus which induces development must in one respect be of quite specific nature, because it brings forth that which corresponds to the region; in another respect, however, it must be of quite general nature, because, in the case of transplant between species the induced structure arises in the manner which is peculiar to the animal from which the tissue was obtained. Figuratively speaking, whatever command is issued by the inductors of the host, the response is executed by the transplanted tissue in the manner provided by the inheritance of its own species. Concerning the mechanism of this response there is as yet no well-founded conception.

Idiosyncrasy to Certain Chemical Compounds

As a part of the Harvard Tercentenary Conference of Arts and Sciences, Dr. Karl Landsteiner, of the Rockefeller Institute for Medical Research, who received the Nobel Prize in 1930 for his discovery of the blood groups, presented an address on "Serological and Allergic Reactions with Simple Chemical Compounds."

Dr. Landsteiner has been engaged for many years on a searching study of the chemical reactions and processes underlying immunity. Animals treated with a foreign protein (antigen) develop specific substances (antibodies) which react chemically with the original antigen if it is again introduced into the body. This phenomenon is fundamental to the development of immunity to foreign bacteria. On the other hand, under certain circumstances an animal treated with a minute amount of a foreign protein may be killed on subsequent injection of a small amount of the same substance, after a suitable time interval. This phenomenon is known as anaphylaxis. The biological mechanism underlying both anaphylaxis and immunity is the same, although its manifestations in the two cases are so radically different. Closely related to anaphylaxis is allergy or excessive sensitiveness to foreign proteins, of which the most familiar example is hay fever.

Dr. Landsteiner, who has probably done more than any other living man to elucidate the chemical nature of antigens and their specificity, devoted his lecture to a discussion of sensitivity to simple chemical compounds, and its relation to the general phenomenon of immunity. "The knowledge of exceptional untoward effects of drugs," he remarked, "or disturbances resulting

from contact with plants or ingestion of food must be as old as medical experience, and histories of such cases date from early times. But the phenomena of idiosyncrasy, in particular drug idiosyncrasy . . . remained wholly inexplicable until the memorable discovery of anaphylaxis. . . . To all appearances this was a clue to the understanding of drug idiosyncrasy. In both cases very small quantities of otherwise innocuous substances produce severe effects in sensitive individuals, and in both instances the sensitivity is strikingly specific.

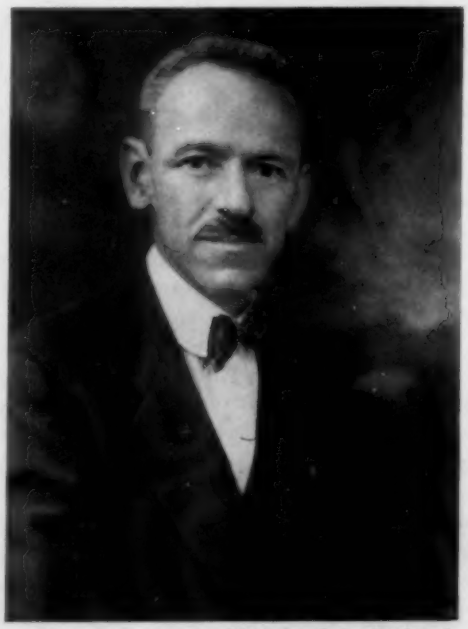
"Still," Dr. Landsteiner continued, "there are significant differences between the two conditions which at once seemed irreconcilable and which led many authors to deny any essential relationship." To clear up these discrepancies, studies were made by injecting simple chemicals repeatedly into the skin of guinea pigs. "In this way definite sensitization effects were obtained with numerous and diversified substances. . . . As regards regularity the method compares with the standard anaphylaxis experiment and the comparison extends to the specificity of the reactions, the duration of the allergic state and the small quantities of substance, *e.g.*, 1/250 milligram which are sufficient to induce an increase in sensitivity."

After a detailed discussion of the significance of his experiments, Dr. Landsteiner concluded: "It may be taken as established from the foregoing that drug idiosyncrasy, in many instances at least, comes into the same category as anaphylaxis."

H. T. C.

WOODS HOLE MEETING OF THE GENETICS SOCIETY OF AMERICA

DURING the first week in September, the Genetics Society of America held its third summer meeting, which was unusually well attended, at the Marine Biological Laboratory of Woods Hole. Of the total registration of 149, 68 were members and the remaining 81 were non-members. As expected, a fair proportion of the attendance came from biologists who spend their summer at Woods Hole. However, 81 came especially for the meeting. Of these 55 were members and 26 non-members. The largest representation of 18 came from Cold Spring Harbor, N.Y. The conference was attended by an outstanding group of geneticists and cytologists, among whom were Drs. W. E. Castle, Chas. B. Davenport, E. M. East, R. A. Emerson, C. E. McClung, T. H. Morgan and E. B. Wilson.



—Photograph by Bachrach

DR. P. W. WHITING

PRESIDENT OF THE GENETICS SOCIETY OF AMERICA; ASSOCIATE PROFESSOR OF ZOOLOGY, UNIVERSITY OF PENNSYLVANIA.

The meeting was opened on September 3 with an evening lecture by Professor Th. Dobzhansky, of the California Institute of Technology, on the "Genetic Nature of Species Crosses" and

OFFICERS OF THE GENETICS SOCIETY OF AMERICA
WHICH HELD ITS THIRD SUMMER MEETING AT THE MARINE BIOLOGICAL LABORATORY AT WOODS HOLE DURING THE FIRST WEEK IN SEPTEMBER. *Left to Right:* ON THE STEPS OF THE LABORATORY: DR. D. F. JONES; DR. L. J. STADLER; DR. R. A. EMERSON; DR. M. DEMEREC.

by a motion picture film of Professor L. R. Dice, of the University of Michigan, showing some types of waltzing and epilepsy in mice of the genus Peromyscus. Dr. Dobzhansky, expanding the usual approach to genetics, said in part: "Organisms are segregated into a finite number of discrete groups, species, which are prevented from interbreeding or exchanging genes with representatives of other similar groups. On the other hand, new gene combinations are constantly arising, due to mutation and hybridization. Evolution may be described as a resultant of interaction of two opposing groups of forces: those tending toward fixation of established gene patterns with proven survival value and those tending to the production of new gene patterns. Fixation is accomplished by a variety of 'isolating mechanisms' preventing free interbreeding." The program consisted of the two morning sessions of round table discussions, each of which was attended by about 160. At each of these sessions the subject was briefly introduced by two speakers, after which followed a general discussion. The first session dealt with "Progress in Cytogenetics," and the second conference with the "Nature of Mutations." The Friday afternoon session was devoted to the presentation of 26 demonstration papers.

M. DEMEREC

THE BRITISH ASSOCIATION AT BLACKPOOL

Two of the greatest scientific groups in the world scheduled their 1936 meetings at famous amusement centers. The British Association for the Advancement of Science convened in the week from September 9 to 16 at Blackpool, a holiday resort in the "north-country" of England, while the American Association is to meet at Atlantic City during the Christmas vacation period.

Blackpool has been termed by Sir Josiah Stamp, president of the British Association, "the world's greatest center of mass amusement and gaiety." The choice of this meeting place was but a phase of the subtle plan underlying the 1936 meetings, the application of science to human affairs. There was a distinct trend in the sectional meetings toward discussions and joint meetings dealing with the relations between science and society rather than isolated papers. The public lectures, delivered in neighboring districts, had a decided popular appeal.

The meetings opened on September 9 at the Empress Hall, Winter Gardens, as Sir Josiah Stamp delivered the presidential address, "The Impact of Science on Society." Two evening discourses were presented to the entire group, Mr. C. C. Paterson on "Science and Electric Lighting," and Captain F. Kingdon Ward on "Plant-hunting and Exploration in Tibet." Following the lecture by Mr. Paterson, the members appropriately had an opportunity to witness the annual autumn illumination of the sea-front, which has increased in splendor yearly during the past fifty years.

The more formal portion of the divi-

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sional programs consisted of addresses by the presidents of the various sections. In the mathematical and physical sciences, Professor Allan Ferguson, on "Trends in Modern Physics"; chemistry, Professor J. C. Philip, on "The Training of the Chemist for the Service of the Community"; biology, Professor H. L. Hawkins, on "Paleontology and Humanity"; zoology, Dr. Julian Huxley, on "National Selection and Evolutionary Progress"; geography, Brigadier H. S. L. Winterbotham, on "Mapping of the Colonial Empire"; economic science and statistics, Dr. C. R. Fay, on "Some Aspects of Commercial Agriculture"; engineering, Professor W. Cramp, on "The Engineer and the Nation"; anthropology, Miss D. A. E. Garrod, on "The Upper Paleolithic in the Light of Recent Discovery"; physiology, Professor R. J. S. McDowall, on "Integration in the Circulation"; psychology, Mr. A. W. Wolters, on "Patterns of Experience"; botany, Mr. J. Ramsbotham, on "The Uses of Fungi"; education, Sir Richard Livingstone, on "The Future in Education"; agriculture, Professor J. Hendrick, on "Soil Science in the Twentieth Century."

The association this year inaugurated a policy in the sectional meetings of "starring in the program those items which deal with aspects of knowledge the repercussions of which on the welfare of the community are direct and important." Starred discussion sessions included those on high voltages, textiles, chemistry and the community, scientific problems of the poultry industry, climate and health, botany and gardening, social and cultural value of science, national nutrition and British agriculture, the strain of modern civilization, Abyssinia and engineering problems of mass-amusement. Discussions not starred dealt with genetics and the race concept, education for rural life, motor-car headlights, sea defences, agricultural geography of the Fylde, earth movements in northeast England and the physical basis of living matter.

The public lectures at outlying Blackpool South, Lytham St. Annes, Poulton-le-Fylde, etc., included Dr. Olaf Bloch, on "The Scope of Photography"; Dr. W. F. Bewley, on "Science and the Glasshouse Industry"; Sir James Jeans, on "Some Recent Advances in Astronomy"; Professor Allan Ferguson, on "Splashes and What They Teach"; Professor C. M. Yonge, on "Common Shore Animals"; Professor J. L. Myres, on "Who Were the Greeks?" and others. The appeal of the meetings was made universal by the inclusion of a special series of lectures for school children.

Besides the unscheduled visits to the nearby amusement park and the open-air swimming bath, thirty sectional excursions of scientific and educational interest were undertaken by members of the association, including expeditions to Garstang, Furness Abbey, the Southport Sand Dunes, Fleetwood Fish Docks, Fylde Farms, the Metropolitan-Vickers Works at Manchester, the Potato-testing Station at Ormskirk and the Mussel-cleaning Station at Lytham.

Sir Josiah Stamp, president of the association, is a world-known economist and chairman of the London, Midland and Scottish Railway. He has served on many committees relative to national and international economics, including the Dawes committee on German currency and finance in 1924 and the Paris committee of inquiry into the reparations question of 1929. "Wealth and Taxable Capacity," with editions in 1922 and 1923, is one of the best known of his books. He has received the honorary degrees of doctor of science from Oxford and Cambridge and the degree of doctor of laws from Harvard University.

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